THE LANCET.

VOL. I.] LONDON, SATURDAY, FEBRUARY 3, 1838. [1837-38.

LECTURES
ON THE
THEORY AND PRACTICE OF MEDICINE;
NOW IN COURSE OF DELIVERY AT THE
THEATRE OF ANATOMY AND MEDICINE, WEBB-STREET, SOUTHWARK.
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&c., &c.

THE NERVOUS SYSTEM.—*Its distinction into cerebral and true spinal. Parts composing the cerebral system. Historic view of the cerebral and spinal nerves. Sir C. Bell's discoveries. Professor Müller's experiments. The true spinal or excito-motory system. Historical sketch. The vis nervosa; Haller's opinion confuted. Detail of experiments on the spinal nerves. Discovery of the reflex function. Anatomy of the excito-motory power. The medulla spinalis is the combiner and disposer of the excito-motory influence.*

GENTLEMEN:—The moment is now arrived when I must introduce to your notice the subject of the NERVOUS SYSTEM and its DISEASES. In doing this, I must necessarily treat at length of my own labours in this department of science and of medicine. It has been my fortune—I cannot, at present, call it my happiness—to have discovered and developed a principle of action equally new and important, and of most extensive application to the physiology and pathology of the nervous system. It is my duty to unfold to you this and the other principles of action in the nervous system.

2. The nervous system has hitherto been subdivided into *two*:—

I. The *Cerebro-spinal*, and

II. The *Ganglionic*, or *Sympathetic*.

3. I shall very soon have occasion to explain to you that the designation, *cerebro-spinal*, is erroneous, and conveys an incorrect idea. It should be simply *cerebral*, that part of the spinal marrow connected with this part of the nervous system consisting merely of the fibres of the cerebral nerves; whilst the true spinal marrow constitutes another and distinct system, of which I shall, from its recent detection, have to treat at great length.

I. OF THE CEREBRAL, OR SENTIENT AND VOLUNTARY SYSTEM.

4. The cerebro-spinal subdivision of the nervous system, or, as it would be more correctly designated, the cerebral, is the system of *sensation* and *volition*. It is the system by which we are connected *intellectually* with the external world. It is that by means of which we feel and perceive external objects, and by which we approach them and appropriate them to our use exteriorly. My fingers being in contact with this book, I feel, I perceive it, and, by an act of volition, I raise it from the table. This apparently simple operation requires *three* portions of anatomy.

5. 1. Certain nerves must proceed continuously, uninterruptedly, *from* the points of my fingers *to* the cerebrum, the centre of this system;

2. The cerebrum must be in a state of integrity; and,

3. Certain nerves must proceed *from* the cerebrum *to* the muscles which are to be called into action.

6. Upon the cerebrum the immaterial and immortal soul sits enthroned, receiving the ambassadors, as it were, from without, and sending forth its emissaries and plenipotentiaries, which convey its sovereign mandates to the external world.

7. There are, then, *two* sets of cerebral nerves; at least there are cerebral nerves having *two functions*. This was distinctly known to Galen. To anatomists it has long been known that the *fifth* cerebral nerve, or the *trifacial*, and *each* spinal nerve, has *two roots*. To Sir Charles Bell the truly splendid idea first occurred, that, as each of these nerves had two functions and two roots, one of the roots might be destined for one of these functions, and the other for the other. This conjecture has been confirmed by experiment. To its great discoverer, Sir Charles Bell; to Mr. Shaw, the brother-in-law, and to Mr. Mayo, the familiar pupil, of Sir Charles Bell, in this country; to Sig. Bel-

lingeri in Italy, and M. Magendie in France, the investigation of this physiological principle, is greatly indebted; it has been completed by Professor Müller.

8. I shall not attempt, at present, to determine the precise degree of merit of each of these physiologists. *None* can rank as a discoverer, in the sense in which I affix that elevated title upon Sir Charles Bell; whereas *all* possess the merit of *pursuing* an inquiry which that distinguished physiologist *originated*.

9. Nevertheless truth compels me to state, that Sir Charles Bell's experiments were defective, and his proof of his own doctrine deficient. He observes, "After delaying long on account of the unpleasant nature of the operation, I opened the spinal canal of a rabbit, and cut the posterior roots of the nerves of the lower extremity. The creature still crawled, but I was deterred from repeating the experiment by the protracted cruelty of the dissection. I reflected, that an experiment would be satisfactory if done on an animal recently knocked down, and insensible; that whilst I experimented on a living animal, there might be a trembling or action excited in the muscles by touching a sensitive nerve, which motion it would be difficult to distinguish from that produced more immediately through the influence of the motor nerves. A rabbit was struck behind the ear, so as to deprive it of sensibility by the concussion, and then I exposed the spinal marrow. On irritating the posterior roots of the nerve, I could perceive no motion consequent in any part of the muscular frame; but on irritating the anterior roots of the nerve, at each touch of the forceps, there was a corresponding motion of the muscles to which the nerve was distributed. Every touch of the probe, or needle, on the threads of this root, was attended with a muscular motion as distinct as the motion produced by touching the keys of a harpsicord. These experiments satisfied me that the different roots and different columns from whence those roots arose, were devoted to distinct offices, and that the notions drawn from the anatomy were correct."— *Nervous System*, 1830. Pref., p. vii, &c.

10. In these experiments a distinction is not made between *excited* and *voluntary* motion. It was proved that the *anterior* roots of the spinal nerves were endowed with a power of *exciting* motion. It still remained to be ascertained whether they were *also* conductors of volition. This was accomplished by Professor Müller, whose results have been confirmed by Sig. Panizzi.

11. Professor Müller observes:—"If we divide in the same frog all the three posterior roots on the left side, and on the right side all the three anterior roots of the nerves of the inferior extremities, sensation in the left leg, and motion in the right leg are destroyed. If we then cut off the foot from the right leg, which retains sensation, but not motion, the frog manifests, by motion in all parts of its body, the utmost pain, but the right leg, which is the seat of the pain, is immoveable. If, on the contrary, we cut off the foot from the left leg, which retains the power of motion, but no sensation, there is total insensibility. This experiment is the most surprising of all, and affords decided, not uncertain, results; because, in the frog, we may cut wholly through the roots of the inferior extremities, the roots being very few but thick."

12. "These experiments place beyond all doubt the truth of Bell's doctrine."— *Handbuch*, p. 629.

13. Thus, then, the proof of the doctrine of Sir Charles Bell, the *second* discovery in physiology, according to Professor Müller, is complete.

14. With the posterior and anterior roots of the trifacial and spinal nerves, Sir Charles Bell associates the posterior and anterior columns of the spinal marrow, as being sentient and voluntary respectively. This doctrine wants the full proof afforded to that of the distinct functions of the two roots of the nerves by the experiments of Professor Müller.

15. The abdominal nervous columns in the *articulata* have been generally regarded as analogous with what has been designated the cerebro-spinal axis of vertebrated animals. Lyonet and Treviranus had partially traced the *third*, or more central and aganglionic column, passing *over* the ganglia of the two peripheral columns; but to Professor Müller is due the merit of having first *traced* and *figured* the third column passing over the upper surface of the ganglia (in the scorpion) in the entire length of the nervous system; and to Professor Grant that of having first distinctly assigned (in his lectures in 1832) to this column its special *motor* function.

16. This latter fact has been certified to me by a pupil who attended Professor Grant's lectures in that year, and who favoured me with a copy of his notes. The doctrine has been very awkwardly, very ungraciously, and very ungratefully published by another pupil, without acknowledgment of the source from whence it was derived. The matter is of the more consequence because that which Professor Grant deduced from analogy and reasoning, has been confirmed by experiment.

I took a lobster and laid bare the nervous columns.

17. I first stimulated one of the aganglionic nerves. The muscles to which it was distributed, and *they alone*, were contracted.

18. I then stimulated a ganglionic nerve. Muscles, both *anterior* and *posterior* to the part stimulated, were excited into combined action.

19. The same event occurred when I stimulated a part of the general or combined nervous column itself.

20. I have thus detailed to you, in as plain terms as possible, the present state of our knowledge of the *cerebral* division of the nervous system. It com rehends,

21. I. Sentient nerves, leading to the posterior roots of the trifacial and spinal nerves, and the posterior columns of the spinal marrow.

II. The cerebrum, the centre of the system.

III. The voluntary nerves issuing from the anterior columns of the spinal marrow and the anterior roots of the trifacial and spinal nerves.

22. A sketch of the cerebral system is given in the subjoined plate.

22. The *lines* which are bracketted with the sketch of the system, are intended to denote the course and ultimate destination of these two sets of nerves, and will be found especially interesting when contrasted with similar lines denoting the operation of another principle,—another system, to which I now proceed to direct your attention.

II. Of the True Spinal, or Excito-motory System.

24. The principle of action to which I allude has long been partially known to experimentors, but it has not hitherto had a place in physiology, or been applied to explain the phenomena of life, of health, or of disease.

25. It was designated the vis nervosa by Haller; it has been called the *motorische kraft*, or *vis motoria*, by Professor Müller, and "excitabilité" by M. Flourens.

26. It has been ascertained that this principle exists in the tubercula quadrigemina, the spinal marrow, and the motor nerves, to the exclusion of the brain and the nerves of sense, the olfactory, the optic, the acoustic; and in the anterior, to the exclusion of the posterior roots of the spinal nerves.

27. It has been supposed by all physiologists that this principle acts *only* in the direction of the branches or fibres of nerves, proceeding *from* their source in the nervous centres *to* their destination in the muscular system. Haller observes, "If a nerve be irritated contraction occurs in the muscle to which that nerve is distributed. If a nerve which is distributed to many muscles, or to a whole limb, be irritated, all those muscles are excited into contraction. Lastly, if the medulla spinalis be irritated, all the limbs are convulsed, which receive nerves *from below* the part irritated, but not the limbs which are situated *above* that part." Haller concludes, "That condition of the nerves which excites motion in muscles proceeds *from above*, or *from* the cerebrum, or medulla spinalis, *downwards*, towards the termination of the nerves." He adds, "So that it appears that the cause of motion acts in the direction *from* the trunk *to* the branches, but not in that *from* the branches *to* the trunk."

28. Professor Müller treats this subject still more at length, and has laid down the following laws in regard to the mode of action of the motor power:—

29. "1. The motor power acts *only* in the direction of the primitive nervous fibres going to muscles, or in the direction of the branches of the nerves, and *never backwards*.

30. "2. The mechanical or galvanic irritation of a part of a nervous trunk does not excite the motor power of the whole nerve, but only of the isolated part.

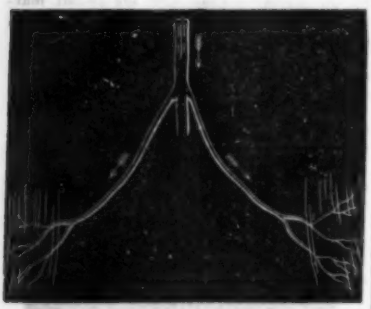
31. "3. A spinal nerve, which passes into a plexus, and assists, with other spinal nerves, in the formation of a large nervous trunk, does not impart its motor power to the whole of that trunk, but only to the fibres which it affords in its course from that trunk to the branches.

32. "4. All nervous fibres act in an isolated manner from the trunk of a nerve to its ultimate branches."—*Handbuch*, I., 656.

33. Thus, if a muscular nerve, or nervous fibre, be stimulated, either mechanically by

the forceps, or by means of the galvanic influence passed across its fibres, the muscle or muscles to which it is distributed are excited into contraction. This fact is represented in the subjoined figure.

34. The same phenomenon is observed, if, instead of stimulating a muscular nerve, the spinal marrow itself be subjected to the action of a mechanical or the galvanic stimulus. All the limbs, the muscles of which receive nerves from *below* the part of the spinal marrow which is subjected to the influence of the stimulus, are thrown into action. This fact is represented in the subjoined woodcut.



35. These are amongst the oldest of physiological experiments. Nevertheless, they are totally without application to physiology. It was surely improbable that a principle of action should exist, so distinct, so energetic, without playing its part in the phenomena of life. You will not, therefore, be surprised to learn that it has, indeed, a wide and extensive influence and agency in the animal economy.

36. But before I proceed to explain to you this extended application of the vis nervosa to physiological phenomena, I must call your attention to another series of experimental facts, equally known to physiologists, yet equally without application to physiology. We are indebted for our knowledge of these facts principally to Redi and Whytt, to Sir Gilbert Blane, to Legallois, and to Mr. Mayo. I shall quote interesting but brief details from each of these authors.

37. Redi removed the head of a large tortoise. It survived thirty-three days. As often as the fore or hind feet were pricked, it moved them with force, and was convulsed.

38. Whytt observes, "A frog, after it has been deprived of its head, when touched, often jumps and moves about for a considerable time, and it is observable that, when the toes of its hind feet are any way stimulated, it draws the feet up to its body; nay, when they are in this situation, if the toes are again irritated, the legs and feet are not extended, but brought still closer to the body. If one of the legs is pulled down from the body and kept extended, no sooner are the toes of this foot wounded than the leg is drawn up to the body as before."—*Works*, 4to., p. 303.

39. Sir Gilbert Blane states, "I took a kitten, a few days old, and divided the spinal marrow, by cutting it across at the neck. The hind paws being then irritated by pricking them, and by touching them with a hot wire, the muscles belonging to the posterior extremities were thrown into contraction, so as to produce the motion of shrinking from the injury. The same effects were observed in another kitten, after the head was entirely separated from the body. In repeating this experiment, I found that when the spinal marrow was cut through between the *lumbar vertebræ* and *os sacrum*, the *posterior extremities* lost their irritability, but the part below it, the tail, retained it." "In an acephalous monster the like *phenomena* were observable. It moved up its knees when the soles of the feet were tickled, it performed the act of suction, passed urine and fæces, and swallowed food."—*Select Dissertations*, p. 262.

40. Legallois states, "In a decapitated frog it is only necessary to touch the skin to excite movements of the limbs;" and he adds, "if we make transverse sections of the spinal marrow, the parts corresponding to each *segment* retain sensation and voluntary motion, but without any harmony, and in a manner as independent as if the whole animal had been divided at the same places."

41. Mr. Mayo has repeated the experiment of Legallois. That gentleman observes, "I have varied this experiment by dividing the spinal chord at once in the neck, and in the back, upon which three unconnected nervous centres exist, and the division of the skin in either part (and especially at the soles of the feet in the two hinder portions), produces a convulsive action of the muscles in that part.—*Mayo on the Roots of the Nerves*, p. 20. Mr. Mayo makes remarks similar to those of Legallois in reference to the distinction of different segments of the spinal marrow.

42. Of all the physiologists who have treated of this matter, Sir Gilbert Blane alone perceived that the movements which have been described were different from those produced by sensation and volition. Neither he, nor any other philosopher has, however, formed the slightest conjecture *what* the principle of action in these movements *really is*. *No one* has imagined what I now proceed to demonstrate, that they, in reality, depend upon the vis nervosa of Haller, acting in directions, however, at variance with the opinions which he had formed, and the laws which Professor Müller has proposed relative to the mode and direction of action of that motor power.

43. In the preceding brief account, I have endeavoured to give a just idea of the state of the science when I began my investigations several years ago. It consisted of mere experiments—mere facts, untraced to any just principle of action, unapplied to the phenomena of life. These experiments could possess little intrinsic value. They assume a very different rank and importance now that I have been able to demonstrate their dependence on the one and identical principle of the vis nervosa, and their extensive application to physiology and pathology.

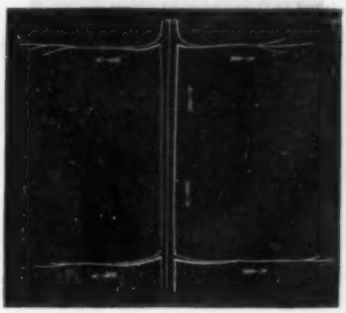
44. Not a step could be taken in this course until the opinion of Haller, and the laws of Professor Müller, relative to the vis nervosa, or motoria, were confuted.

45. No second step could be taken until it was shown that the power which was only known to act upon external and cutaneous surfaces, in experiments, acts also upon *mucous internal* surfaces in the living animal economy.

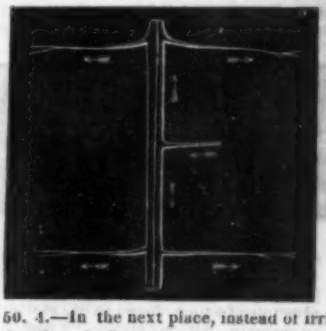
46. These two objects I have accomplished in a series of experiments which I now proceed to detail.

47. 1.—The head of a turtle being removed, to remove sensation and volition, I denuded and divided the spinal marrow in the dorsal region; I then irritated the *lower* end of the *upper* portion by a needle, the forceps, and galvanism; I produced movements in the *superior* extremities. The motor power had acted in a retrograde direction. This experiment is represented in the subjoined figure:—

48. 2.—In another decapitated turtle, I laid bare the spinal marrow in the dorsal region, and stimulated it as before. I produced motions in *both* the superior and the inferior fins. This experiment, represented in the subjoined sketch, combined the experiment of Haller and my own, and proves that it is the *same* motor power which acts in *both*.



49. 3.—Instead of denuding the spinal marrow, I now exposed a spinal intercostal nerve in the decapitated turtle, and stimulated it as I had done the spinal marrow itself in the former experiment. I produced similar movements in both the superior and inferior extremities. In this experiment the motor power again acted in a retrograde, or in an *incident* course, into the spinal marrow, and both upwards and downwards into both extremities. This experiment is pourtrayed in the subjoined woodcut:—

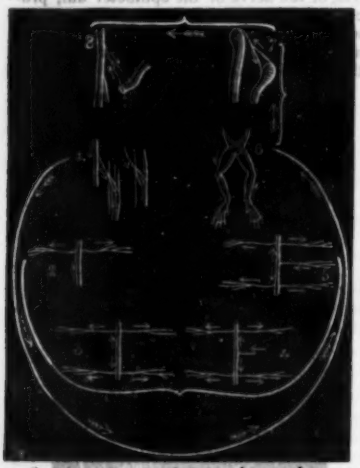


50. 4.—In the next place, instead of irritating the spinal marrow, or the nerves, I irritated the *cutaneous* surface to which those nerves are distributed, in a decapitated turtle. Precisely the same phenomena ensued; both superior and inferior extremities moved. The same effects were observed when I irritated the extremities of any of the fins. But this is identical with the experiments which we have quoted from Whytt, Legallois, &c., and referred by them and by all physiologists, with one exception,

to sensation. It is sketched in the following woodcut:—

51. I have thus traced the operation of the vis nervosa of Haller, in new directions, until I have shown that its last mode of action is *identical* with that of the second series of experiments to which I have alluded. It now remains to point out the *application* of this principle to the phenomena of the animal economy.

52. Now, on irritating the border of the glottis in an animal from which the brain has been removed, the larynx is closed; on irritating the border of the anus, the sphincter is firmly contracted. By an extended series of experiments I have proved that these, and a multitude of other physiological phenomena, to be detailed hereafter, depend upon the action of the spinal marrow; and thus, I think, my demonstration is complete. It is given in the annexed woodcut; for it is most important as denoting the progress of this inquiry:—

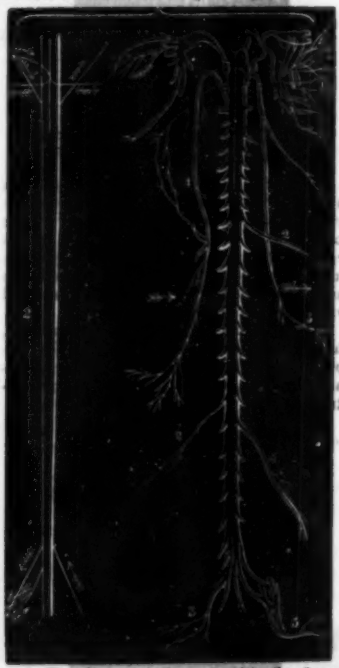


53. This sketch plainly shows the progress of this inquiry. The action of the larynx and sphincter ani are shown in the most distinct manner to depend upon the vis nervosa of Haller. The statement must be generalised so as to include *all* the *orifices* and *all* the *sphincters* of the animal frame.

54. But there are further applications of the excito-motory power to physiology.

55. These remarks lead me to observe that *all* the acts of this system are the result of *excitation*, by stimuli applied to *nerves* which proceed to the *spinal marrow*, whence other nerves take their origin, and pursue a *reflex* course to the parts to be moved.

56. The *system* of *incident nerves*, of the *true spinal marrow*, and of *reflex nerves*, is, like the agency carried on through it, new to physiology. It presides with its own power over the acts of ingestion and expulsion in the animal economy,—over the orifices and sphincters of the animal frame. It is represented in the subjoined plate:—



57. The lines bracketted with the figure denote the varied course in which the excito-motory power moves and acts, and effectually disproves the idea of mere *segments* of the spinal marrow. If that idea were true, we must, at least, admit of segments in longitudinal as well as transverse directions.

58. The subjoined figures represent the course of incident nerves—as the trifacial,

the spinal centre, and the course of reflex nerves—as the facial, through which the excito-motory property acts, in the excited closure of the eyelids on touching their borders or lashes in the case of apoplexy:—

59. *Five* circumstances are required in this and every instance of an excito-motory act:—1, *an excitant*; 2, *an excitor nerve* continuous to the nervous centre; 3, the integrity of *that centre*; 4, a *motor nerve* continuous to the muscle to be excited into contraction; and, 5, that *muscle* endowed with perfect irritability.

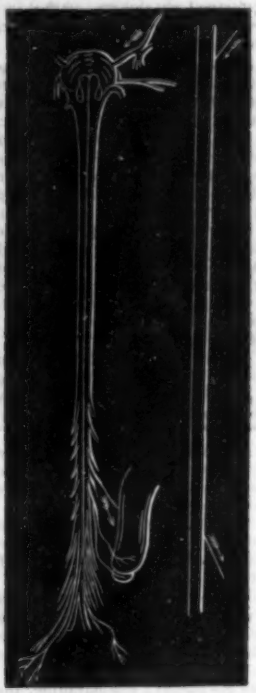
60. If any part of this arc be interrupted, the phenomenon ceases instantly. The subjoined figures represent the spinal centre—the key-stone of the arc, as it were, destroyed:—

61. The first of *these* subjoined figures represents the experiment in which the *incident nerve alone*, the second that in which the *reflex* nerve alone, and the third that in which *both* are severed:—

62. These facts prove that certain incident nerves, as well as the spinal marrow and motor nerves, are excito-motory; and they establish a *class* of such nerves previously unknown to physiologists, or confounded with sentient nerves.

63. The two next figures represent an incident nerve, as the trifacial, or the nerve which supplies the border of the larynx, or of the sphincter ani, proceeding to the upper or lower portion of the spinal marrow; and a reflex nerve, as the facial, the pneumogastric, or the nerve of the sphincter ani, proceeding from that organ to and from points which are nearly on the same parallel. They may be said to denote distinct *segments* of the spinal marrow.

64. But of the subjoined figures the one placed on the left denotes the *direct*,—



the one in the opposite column the *retrograde*,—course of the excito-motory influence along the spinal marrow, as observed in experiments in physiology, and in the effects of diseases and of remedies, and disprove the idea of the excito-motory phenomena being restricted to *segments* of the spinal marrow, at least *transverse* segments; and prove the existence, at least, of segments taken in a longitudinal direction.

65. From the experiments which have been detailed, we may, I believe, infer the existence—

66. 1.—Of a *true spinal marrow*, *physiologically* distinct from the chord of intra-spinal nerves.

67. 2.—Of a *system of excito-motory nerves*, *physiologically* distinct from that of the sentient and voluntary nerves.

68. 3.—Of a nervous influence—the excito-motory power—operating in directions *incident*, *upwards*, *downwards*, and *reflex*, with regard to the true spinal marrow, the centre of this excito-motory system.

69. The entire medulla spinalis in the *vertebrata* consists, then, of *two* portions, so intimately blended together, indeed, as not to be easily separated by the anatomist, and, perhaps, only to be distinguished by physiological experiments and pathological observations. The *first* of these is the intra-vertebral *chord* of sentient and voluntary *nerves*, which proceed *to* and *from* the *cerebrum* as their centre. It is represented in the figure § 22. The *second*, which may be denominated the true spinal medulla, is distinguished by being *excito-motory*, and is the axis of a peculiar system of excitor and motor, or excito-motory nerves, generally, but perhaps not invariably, blended with the former. This is represented in the sketch given § 57.

70. The close combination of these two portions of the nervous system, in the *vertebrata*, is the consequence of the necessity for the several pairs of compound nerves being *inter-vertebral* in their exit from the spinal canal. In the *articulata* this necessity does not exist, and the two systems may, therefore, be anatomically, as well as physiologically, distinct. Indeed, I think I have ascertained that, whilst the ganglionic nerves in the lobster are incident and excitor, and the columns both direct and retrograde in their influence, the aganglionic nerves are purely motor (as Professor Grant

first conjectured), and direct in their mode of action.

71. These observations lead naturally to the question,—Is there, in any class of animals, a distinct *anatomy* of the excito-motory power? Are there excitor nerves distinct from nerves of sensation? Are there motor nerves distinct from nerves of volition?

72. In the first place, I may observe that the olfactory, optic, and acoustic nerves are nerves of *sense* only, and destitute of excito-motory power; so are the cerebrum and cerebellum, the former of which is probably the centre of the sentient and voluntary system. Is there a pure *voluntary* nerve? a nerve which conveys the acts of the will, without possessing the motor, or excito-motory power? It appears to me, that *one* such purely voluntary nerve only exists, for every muscle of the animal power, with one exception, seems to require *tone*, which is a result of the excito-motory power, conveyed by motor nerves, probably involved, in general, in the same neurilemma with voluntary nerves. This power acts during *sleep*, in *all* muscles, *except the levator palpebræ*, and perhaps the *four recti* oculi.

73. But as there are purely sentient nerves, it may be a question, whether there be purely excitor nerves. Such a nerve probably does not exist absolutely in health. An experiment made by the late Mr. Broughton, Mr. Field, and myself, in 1835, led to the conclusion that the *pneumogastric* nerve is destitute of sentient property. This nerve is certainly the least sentient, and the most purely excitor, of any in the class *vertebrata*. In certain cases of disease we, however, observe the sentient power annihilated, whilst the excito-motory still continues; this occurs in those diseases of the brain which destroy the sensibility of the face; the excito-motory property may remain, and the eyelash and the nostril be as susceptible of stimuli as ever. In the experiments in which the cerebrum, the *centre* of the sentient and voluntary system, is removed, and in diseases, and in other experiments in which the spinal marrow is disorganised, or divided, the phenomena which remain are entirely of the excito-motory class. Sentient and voluntary nerves are blended with the excitor and motor nerves, but their functions are suspended when the influence of the centre of their system is cut off. The centre of the excitor and motor nerves being the appropriate portions of the spinal marrow itself, the functions of these nerves remain.

74. Still the two sets of nerves are generally blended anatomically. If they be distinct in any class of animals, it is probably in the *invertebrata* (see § 70), and especially in their lowest forms, in which sensation and volition are nearly extinct, and the animal lives a sort of excito-motory life only.

75. But if the existence of a *distinct* anatomy of the excito-motory system be doubtful, that of the *blended* anatomy, and that of of the distinct physiology, pathology, and therapeutics of this system, are perfectly obvious.

76. I think I may now regard the proof as quite complete, that the principle formerly designated the vis nervosa, and that which operates in producing that series of actions, which have been designated instinctive, automatic, sympathetic, &c., but which I propose to designate excito-motory, are one and the same. The incident, retrograde, and reflected courses, and the combined forms in which it operates, are at variance with the laws of its operation, deduced from the facts formerly known by Professor Müller, and afford the *type* of the extensive series of physiological, pathological, and therapeutic phenomena to which I have alluded.

77. The experimental fact noticed and represented § 50, gives the type of all those physiological phenomena in which the excito-motory property acts, first in an incident, and then in retrograde and reflex directions, and in *combined* modes, as we observe in the excited acts of ingestion and expulsion, and in the action of the orifices and sphincters.

79. The same fact represents the effects of dentition. The experiment detailed and represented § 49, affords us the type of traumatic tetanus.

80. The therapeutics of agents which operate through the excito-motory system, are still nearly unknown, and require a careful investigation. Strychnine, besides acting on the general excito-motory system, is apt to affect the larynx; cantharadine, the cervix vesicæ, &c.

81. In *all* these actions the medulla oblongata, or the medulla spinalis, is the special COMBINER and DISPOSER of the excito-motory influence, in producing those complex effects and operations which we observe in deglutition, respiration, &c.

82. This brief view of the excito-motory system appears to me to consist in a series of experiments and observations, rather than of deductions. It is, therefore, scarcely liable to error; its originality is obvious; its importance will be made manifest as we proceed.

83. We are now prepared to trace the excito-motory power, for so I have designated the principle of action variously termed the *vis nervosa*, the *vis motoria*, the *excitabilité*, in

I. *A special anatomy;*
II. *A special physiology;*
III. *A special pathology;* and
IV. *Special therapeutics.*

These subjects will be discussed in my next and subsequent lectures, previously to which I must make a few remarks upon the *ganglionic* division of the nervous system.

LECTURES
ON
MATERIA MEDICA AND THERAPEUTICS;
NOW IN COURSE OF DELIVERY
AT THE
WINDMILL-STREET SCHOOL OF MEDICINE.
BY
GEORGE G. SIGMOND, M.D.

CALUMBA, COCCULUS PALMATUS.

COCCULUS PALMATUS.—*Description of the plant; characters and properties of the root falsifications practised by the importers; mode of detecting the spurious calumba; preparations of the drug; its medicinal virtues.* ANGUSTURA BARK.—*Botanical description; the true plant first pointed out by Dr. Hancock; efficacy of the Angustura in some fevers; experiments of Dr. Hancock; spurious Angustura; dangerous effects and accidents thereby produced; mode of detecting the false bark.* DORSTENIA CONTRAYERVA.—*Description of the plant; preparations used; their medicinal value.*

GENTLEMEN:—The roots of several of the menispermaceæ, or the cocculus tribe, are possessed of tonic and febrifuge powers, and have considerable bitterness. The cocculus platyphylla, the cocculus cinerascens, peltatus, crispus, fibraurea, bakis ovalifolia, and the species of which I am about to speak to you, the palmatus, are all of them esteemed for the medicinal virtues they possess, and are used in the cure of fever in those countries in which they are indigenous. The roots of some of this tribe seem to have still further claims to our notice; for some of them are diuretic and aperient, and one has lately been introduced into our Pharmacopœia, as very valuable in diseased states of the organs destined for the urinary secretion: the cissampelos pareira, or pareira brava. This medicine has been lately revived amongst us, and is brought to this country sometimes mixed with roots of different species which are inferior to it in activity, such as the cissampelos mauritania, and abuta, candicans, and rufescens, which have more or less of the same power of acting upon the urinary organs. I have had occasion to mention to you the effect of the seeds of one of the species,—the menispermum cocculus,—as being most remarkable for the possession of narcotic properties, which seem to reside in a principle called picrotoxia.

The cocculus palmatus, or calumba plant, grows in the thick forests that cover the shores about Oibo and Mozambique, on the east coast of Africa, and was, for a long time, a chief article of export of the Portuguese from Mozambique; but it was for a long time supposed to be a native of the island of Ceylon, and to take its name from the principal town, but it is now ascertained that it is called amongst the Africans kalumb. M. Fortin, when trading on the coast of Africa, obtained an entire off-set of a larger size than usual; on his proceeding to India he presented it to Dr. Anderson, of Madras, who immediately planted it, and had the satisfaction of rearing the plant; it proved to be a male. We should, however, have been without a proper knowledge of the female, had not Dr. Hooper, of Glasgow, obtained from Professor Boyer a full description of it, which he published together with a delineation of it coloured by a lady in the Mauritius. It is in the "Botanical Magazine" for the year 1830. From this it appears that the root is perennial, composed of a number of fasciculated, fusiform, somewhat branched, fleshy, curved, and descending tubers, of the thickness of an infant's arm, clothed with a thin brown epidermis, marked towards the upper part, especially, with transverse warts; internally they consist of a deep yellow, scentless, very bitter flesh, filled with numerous parallel longitudinal fibres, or vessels; the stems are annual, herbaceous, about the thickness of a little finger, twining, simple in the male plant, branched in the female, rounded, green; in the full green plant below, thickly clothed with succulent longitudinal hairs, which are tipped with a gland. The leaves are alternate, the younger ones thin, pellucid, bright green, generally three-lobed; older ones remote, a span in breadth, nearly orbicular in their circumscription, deeply cordate, five to seven-lobed, generally three-lobed, the lobes entire, often deflexed, waxy on the surface and margin, dark green above, paler underneath, hairy on both sides, with prominent nerves and supported on round hairy footstalks, about as long as the leaves. In the male plant the racemes are axillary, solitary, or two together, drooping, about as long as the petiole, compound, clothed with glandular hairs, and having at the base small deciduous bractens. The calyx is smooth consisting of six ovate, acute, nearly equal leaves arranged in a double series; the corolla is pale green, consisting of six petals arranged round a central orbicular disk or gland, in a single series; the filaments are six, with four-celled anthers, the cells opening internally, and filled with oblong grains of yellow pollen. In the female plant the racemes are also axillary, solitary, simple, shorter than those of the male; the pedicles are furnished with minute caducous bracteas The petals are six, rarely eight, green, glabrous, shorter than the germens, and recurved at the extremity. The pistils are three, free, of which two are generally abortive, and contain one ovule. The style is very short, and the stigma has several

spreading points; the fruit is drupaceous, or berried, about the size of a hazel-nut, densely clothed with long spreading hairs, which at their extremity are tipped with a black gland; the seed is subreniform, clothed with a thin black shell, transversely striated.

It is about the month of March that the roots are dug up; it is only the off-set from the main root that is cut away, the principal stock being left; great care is taken as to the selection of those that are not too fibrous; as soon as it is somewhat dry, it is cut into the slices in which you see it, and hung upon ropes in the shade until all the moisture contained is perfectly evaporated; that which breaks short is considered very good; that which becomes at all black is rejected as unfit for exportation. You will observe that the specimens which are placed before you are in round pieces; these are the results of the slicing the roots transversely. There is an external bark of a brown wrinkled appearance. The interior part is of a light yellow colour: it is much shrunk up from the way in which the exsiccation takes place; the texture is spongy, there are rays converging which are of somewhat a darker colour. Those sections which weigh most, which are not worm-eaten, which are heavy, and of a bright yellow colour, are evidently the best. It is liable to many adulterations, both in this country, and in the places from which it is exported. The root of the red-berried bryony, bryonia dioica, tinged yellow, with a little of the tincture of columba, is occasionally made up to resemble it; and the root of an American plant, the Fraseri Walteri of Michaux, is imported, and sometimes sold as the true calumba; from which, however, it is to be distinguished by its great sponginess, its lighter texture, its whiter appearance, the absence of the wrinkles, and the rays, but, above all, by its taste, for it has by no means the aroma and not disagreeble bitterness of the columba; besides which the first impression that it makes is somewhat sweetish. Bigelow, in his "Treatise on the Materia Medica," has pointed out these distinctions; and Stoltze, in the "Pharmaceutic Annual of Berlin," has given us a test, the infusion of galls, which, he observes, gives no precipitate with the infusion of the fraserin. There is also a spurious columba sent from the coast of Africa to France, which contains no starch, and is, therefore, easily detected by iodine, which does not affect its colour; it likewise turns black with sulphate of iron, and disengages ammonia by the action of caustic potash. Planche, whose labours are to be found in the "Bulletin de Pharmacie," has shown that columba contains starch, a yellow bitter matter, an animal substance, a quantity of volatile oil, salts of lime and potass, oxide of iron and silex, and it is in the yellow matter that the peculiar properties of the drug reside.

Our preparations are a tincture and an infusion, which have all the useful qualities that are inherent in the root, and both of them form useful medicines. The infusion is made thus:—Take of columba, sliced, five drachms, of boiling distilled water a pint; macerate for two hours in a vessel, lightly covered, and strain; of this an ounce or two may be given. Mr. Phillips observes, that it contains no astringent matter. It must not be long kept, as it is soon rendered unfit for use. The tincture is made by macerating, for fourteen days, three ounces of sliced calumba in two pints of proof spirit, and then straining; a drachm or two is the usual dose. The powdered root, too, is occasionally given in substance, and about a scruple, two or three times, is taken either alone, or in combination with some other of the tonics, or febrifuges. The knowledge we have of the value of the calumba root is principally to be ascribed to Dr. Percival; his essays, medical and experimental, contain some very valuable dissertations on many subjects, and amongst them is one entitled, "Observations and Experiments on the Calumba Root." It has been found that modern practice has verified the inductions of Dr. Percival, and the diseases in which he recommended it are frequently controlled by its use. It is in diarrhœa, and more especially that which proceeds from an inordinate secretion and discharge of bile, where astringents would be hurtful, it seems to correct the irregular action, and to diminish gradually the increased action of the liver. In bilious colic, where there is severe sickness and vomiting, it has been, also, much prized. It was prescribed with very considerable advantage by Mr. Johnson, in the East Indies, who had the care of a hospital ship, during the prevalence of the cholera, and it sooner checked the vomiting than any other remedy which was used. It has been employed with success in bilious fever; and Dr. Haygarth, in a fever of that kind, which had been epidemic at Nantwich, and other parts of Cheshire, found it to supply the place of cinchona bark, correcting the bile, resto ing the proper tone of the stomach, and of the whole habit; it also prevented relapses, to which, in that fever, the patients were very liable.

During dentition children are very apt to be subject to severe vomiting and diarrhœa, and very often instant relief is procured by the calumba root, when other efficacious remedies have been tried in vain. In enfeebled states of the stomach, brought on by dissipation, by hard drinking, where the appetite fails, where nausea and flatulence distress the individual, this may be administered, combined with the common gentian, or with chirayita, infused in a little

Madeira wine; and, during its use, the tincture of rhubarb may occasionally be prescribed as a warm and strengthening purgative. Vomiting, arising from disordered function, from depraved secretions of the gastric and pancreatic juices, when it is totally independent of any diseased action of the kidneys, which is more generally the cause of habitual vomiting than anything else, is essentially relieved by the occasional use of the calumba root, and then it is very necessary to combine it with warm cordials, with the diffusible stimuli, or any of the aromatic bitters. In that highly distressing state of stomach which occurs in the earlier stages of pregnancy, where nausea and sickness are the constant attendants of the early morning, where the sight of almost any food is very apt to produce vomiting, and where the want of appetite, or a depraved desire for food occurs, I have very constantly recommended the infusion of calumba, with very great relief to the patient, more particularly where the bowels are kept in a gently relaxed state by magnesia, which, by its absorbing power, and its neutralisation of the acid which is at this period generated, becomes a mild and gentle aperient, and at the same time alleviates the disordered sensations that are so usually complained of.

Calumba root has the power of retarding putrescency, and has been observed to moderate alimentary fermentation without suspending the process of digestion. As its taste very much resembles mustard, Dr. Percival was led to make some comparative experiments, with infusions of calumba, of mustard, and of chamomile, and they were in favour of the calumba root. He was, therefore, led to the conclusion, that it had the advantage over other bitters in disorders of the stomach attended with "a violent fermentation of the food." Certainly, where there is an extrication of a gaseous flatus, and much acidity, it has very considerable powers, and may be safely taken by the dyspeptic a few minutes before a meal. It appears neither to increase the fulness nor to affect the regularity or velocity of the pulse. It is particularly serviceable where a milk diet has been found necessary for the nutrition of the body, for it prevents the acidity which is so generally the consequence, and likewise diminishes the tendency to flatulence.

Dr. Denman has found it preferable to bark in the low stage of puerperal fever. As for any benefit in phthisis pulmonalis and in hectic fever, I believe little to be derived from it further than that the organs of digestion are better capable of bearing it as a tonic than they would the Peruvian bark, which, in some instances, produces diarrhœa. The chewing the root has been recommended in dyspepsia, in pyrosis, and in gastrodynia; altogether it may rank as an agreeable tonic, free from the objections which are raised to the stronger bitters, and sufficiently powerful to strengthen the system where no very acute disease, nor any very materially altered state of organs has occurred.

CUSPARIA, OR ANGUSTURA BARK.—GALIPŒA CUSPARIA.

There are among the rutaceæ, or rue tribe, several species, characterised by great bitterness, and by their febrifuge powers. Amongst these there is a bark possessed of these good qualities, which grows abundantly on the river Carony, in South America, and has been prized by the Capuchin Friars, who form the different missions of Upata, of Capassui, and Magna Græcia. It is called the quina de la Guayna, or de la Angustura, as it came from Nueva Guayna, or Angustura. This bark has been said to be the produce of different trees. It has been ascribed to brucea ferruginea to two species of magnolia, to the bonplanda trifolinta of Willdenow, the cusparia febrifuga of Humboldt.

In the splendid work upon equinoctial plants, by Humboldt and Bonpland, is described the bonplanda trifoliata, as an elegant evergreen, which rises to fifty or sixty feet in height, its trunk covered with a grey bark; its wood is bright yellow, and susceptible of a high polish. It has numerous branches; the younger ones covered with smooth bark, of a bright green colour, and studded with small greyish tubercles. The leaves are ranged alternately on the branches; they are about two feet long, and composed of three oblong leaflets, attached to a common petiole, which is from ten to twelve inches in length. The flowers are produced in a terminal raceme, composed of alternate peduncles, bearing from three to six flowers each; the calyx is inferior, persistent, five-toothed, and tomentose; the corolla funnel-shaped, and composed of five petals, which appear as one tube. The nectary consists of five oblong glandular bodies, covering the base of the germen. The stamens are shorter than the petals, and are furnished with oblong yellow anthers. The germen is superior, and composed of five ovate hairy ovaries, with a single style, and five oblong blunt fleshy, green stigmas; the capsules consist of five oval bivalve capsules, each inclosing a single seed. This description, together with a figure which represents a raceme, or spike of flowers, of the bonplandia, with a leaf, in outline, considerably reduced, you will find in Stephenson's "Medical Botany," taken from the work to which I have alluded.

Dr. Hancock, however, had actual opportunities of seeing the true Angustura bark. In a paper read before the *Medico-Botanical Society*, in 1828, he gave his observations on the external appearance of the

plant, and the prominent differences between the plant he had seen and the one described by Humboldt and Bonpland. The gold medal of the Society was awarded to him for his discovery, and that he is fully entitled to the merit of pointing out the real tree from which the bark is obtained, there can be no doubt. Humboldt himself, who has spoken in honourable terms of Dr. Hancock's labours, has not controverted the statements advanced by our learned and indefatigable countryman, who having travelled repeatedly, and resided during several months (particularly during August and September, 1816), in the missions of Carony, and sketched a map of the district, had had an opportunity of seeing many thousands of the bark trees, and of examining numerous specimens on the spot. In the course of his observations he remarked that it would have been impossible for any botanist, however expert, to recognise the Angustura bark tree with the assistance of any one of those works, into which its descriptions have all been transcribed from that of Baron Alexander de Humboldt, and his scientific coadjutor, M. Aimé Bonpland. He was informed by MM. Ravigo and Jose Terreas, with whom the travellers lodged at Angustura, that they did not visit the missions of Carony, but sent an Indian, who returned with a sample (*muestra*) of the leaves, but, much to their disappointment, without flowers. It is, therefore, probable that their descriptions refer chiefly to specimens which they observed in the province of Cumana, where a *species* of the genus to which the Angustura bark tree appertains may grow to the size mentioned.

He was never enabled to learn from what source the illustrious travellers above mentioned derived the name *cuspare* for the Carony bark tree. He resided for three years and a half at St. Thomas de Angustura, in Spanish Guiana, whence he made several excursions amongst the missions of Carony, and the tracts inhabited by Indian tribes between them and the mountains of Parime, but never once heard the term used; the vernacular name among the aborigines of this part of Guiana being *orayuri*; and among the Spaniards and Creoles, it was known by the name of *cascarilla*, or *quina de Carony*. The cuspa, however, which is known as a tree of Cumana, has a bark that is bitter, and of a yellow tint; and although it is much lighter, nauseous to the taste, and altogether different from the orayuri, it is fancied by the inhabitants of Cumana to be allied to the Carony bark tree; at the same time they acknowledge its virtues to be much inferior.

The Angustura bark tree grows in abundance on the mountains in the neighbourhood of St. Joaquin de Carony, situated between the 7th and 8th degrees of northern latitude. It is also well known in the missions of Tumeremo, Uri, Alta Gracia and Cupapui (as correctly mentioned by Humboldt), which are the southern and back missions of the Orinoko, at a distance of upwards of 200 miles from the sea. It lines the roadside, in many places, between the missions of St. Antoni and Villa Upatu. It delights in a rich soil, and flourishes at the height of between 600 and 1000 feet above the level of the sea. It seldom or never exceeds the altitude of 20 feet, the usual medium being about 12 or 15 feet. The diameter of the trunk, which is tolerably erect, is from three to five inches. Branches scattered over the whole tree without much order. Bark, smooth and externally gray. Leaves, placed, for the most part, alternately on the branches, composed of three folioles. Flowers, numerous, borne towards the extreme part of long spikes or racemes, which are both terminal and axillary. Bracteæ, lanceolate, acute, in pairs. The flowers also have a peculiar, not the most pleasant, odour. Calyx, monopetalous, bell-shaped, five cleft, hairy, rough, inferior, and persistent; green, about one-fourth of the length of the corolla. Corolla, somewhat curved prior to expansion, tubular, bursting from the centre. Stamina, two. Filaments flat, inserted into the two longer petals at the mouth of the tube, considerably shorter than the nectaria. Anthers large, linear, erect, longer than the filaments, four-channelled, two-celled. Pistillum, consists of a five-lobed depressed germ, immersed within a coriaceous receptacle; a simple, filiform style, hairy at the middle, longer than the tube, and a capitate entire stigma. Pericarp, consists of five bivalve capsules, of which two or three are commonly abortive, resembling short legumes, gibbous. Seeds, two to a capsule; one of them often abortive, round, black, the size of a small pea, fastened near together by minute pedicles within a chaffy envelope, which is again surrounded by a strong elastic perisperm or arillus, which is horny, bivalve, bursting with violence, and dispersing the seeds it contains to a considerable distance.

The Angustura bark tree flowers in vast profusion during the months of August and September, when its elegant white blossoms add greatly to the beauty of the scenery. Its seeds ripen in October and November.

Decandolle, in arranging the species of galipœa, has adopted the cuspari as the specific name, and the London College has followed his example; the pharmacopœias of the United States have, however, adopted the authority of Dr. Hancock.

In the years 1816 and 1817 there prevailed in the district of Orinoko, and particularly at St. Thomas de Angustura, a malignant bilious intermittent fever, which proved fatal to great numbers of the inhabitants as well as to foreigners. In the latter, it assumed

the form, in many cases, of true yellow fever, with *vomito prieto*.

Dr. Hancock had the appointment of *Medico de Sanidad* in the harbour, which is about 260 miles up the river, and had an opportunity of observing this disease in all its various shapes. He had also the care of the *Military Hospital* in 1817, and had seldom less than 60 or 70 patients with fever, dropsy, and dysentery. The number of hydropic patients was almost incredible. It was distressing to see them dying along the streets of Angustura from the effects of fever and want of food, the town being besieged by the patriot forces under General Bolivar.

In March, 1817, the mortality increasing, the stock of cinchona was soon expended, and there was no other resort but to the quina de Carony, of which there was a large supply in the town. It was prepared nearly as prescribed by those who were there termed *curiosos*, or the native doctors.

Into a large jug, containing about six gallons, was put one pound of coarsely-powdered bark, with an equal quantity of brown sugar, the jug was nearly filled with boiling water, and about four ounces of wheaten bread added to hasten fermentation. It was then stopped close, placed in the sun, and shaken frequently. As soon as fermentation was well begun, it was considered fit for use, and administered in the quantity of from four to six ounces to the dose, three or four times a day.

The success of this seemingly odd preparation was very remarkable. The irregular paroxysms of fever were suspended on the second or third day after commencing its use. The number of deaths of patients from fever was soon diminished to one-fourth of that which before fell victims to this dreadful scourge; though prior to this time it was gradually on the increase.

Dr. Hancock, at first, conceived that fermentation might injure the remedy, but had subsequently every reason to suppose, that the evolution of the carbonic acid rendered the remedy more energetic, and more grateful to the palate and the stomach.

It was not long before he perceived the efficacy of the fermented infusion in dropsy. This naturally induced him to give the same remedy as a tonic to those patients who were simply dropsical or without fever. Its power in those proved more striking and decided than anything he ever witnessed before in medicine.

He had thus no reason to regret the exchange he had from necessity made, for the Angustura bark was found to be greatly superior to the Peruvian bark.

It was in the year 1788 that the bark of the tree was first imported from Dominica, and our acquaintance with the medical powers by which it is distinguished is derived from Brande, Lettsom, and Winterbottom, who have given us the result of their experience; and more recently Dr. Hancock has spoken of it, and pronounced that, "from ample experience of the virtues of the bark, it is one of the most valuable febrifuges we possess, being adapted to the worst and most malignant bilious fevers, while the fevers in which cinchona is chiefly administered, are simple intermittents, for the most part unattended with danger.

Sometime since there was introduced into Europe a spurious Angustura, or cusparia, which was a source of considerable mischief; it was said to be the produce of the brucea ferruginea, a native of Abyssinia; it approached very nearly to strychnia in its intensity of action; so many were the disasters to which it gave rise, that, in some parts of the Continent, all the bark that had been imported was ordered to be burnt. The symptoms that presented themselves, where a portion had been taken, bore a very strong resemblance to those I have detailed to you as following upon the injudicious employment of nux vomica. In Hufeland's Journal is to be found a paper upon this subject, by Professor Emmert, and he has minutely described the effects. Professor Mare, of Paris, owing to a mistake, took a small quantity of the spurious Angustura, for the cure of intermittent fever. An infusion of it had been made, of which he only drank three-quarters of a liqueur-glass full, and for two hours he suffered from a train of very alarming symptoms, which fortunately were alleviated by the administration of ether and of the tincture of opium. He was, at first, seized with nausea and pain in the stomach, there then followed a determination of blood to the head, giddiness, singing in the ears; his vision became obscure, his limbs stiff; every attempt at movement produced pain; locked jaw supervened, and his articulation was incoherent, and he was almost incapable of uttering any sounds. The sensibility, it would appear, is materially affected even during the absence of those paroxysms, which bear so strong a resemblance to those that occur where strychnia has been taken; for, in one instance, a lad who had died in consequence of having taken it, implored the physician not to touch him, as it threw him into the most violent fits. Christison mentions the experiments made by Emmert to show that the poison acts on the spine directly, and not on that organ through the medium of the brain, for if an animal be poisoned by inserting the extract of the false Angustura bark into its hind leg, after the spinal chord had been severed at the loins, the hind legs as well as the fore legs are thrown into a state of spasm; or if the medulla oblongata be cut across, and respiration maintained artificially, the usual symptoms are produced over the whole body by the administra-

tion internally, or the employment externally, the only material difference being that they commence more slowly, and that a larger dose is required to produce them than where the medulla is not injured; on the other hand, when the spinal chord is destroyed suddenly after the symptoms have begun, they cease instantaneously, although the circulation goes on for some minutes. The symptoms that are produced certainly lead one to believe, that it is to the principle called brucia that these effects were owing; this principle is twenty-four times less powerful than strychnia, according to Andral; but the Angustura, or, at least, the false bark, is very powerful, for we learn from the "Toxicologie Generale," that Orfila destroyed a dog in less than two hours with eight grains. Some botanists have thought that this fearful drug is obtained from the solanum pseudo quina, or the quina da campo of the Brazilians. Of course it becomes a matter of the deepest importance to us, to distinguish the medicinal cusparia from the poisonous; and in the twelfth volume of the "Edinburgh Medical and Surgical Journal" will be found an accurate detail of these differences. It is brought to this country in casks. It is in pieces of different lengths, some are almost flat, others are quilled; externally it has a rough, gray appearance; internally it is of a brownish yellow colour, powdered it is pale yellow; it is very easily reduced to powder. The following have been given as the characteristics by which the true bark may be known, when compared with the false. The true has a strong and disagreeable smell, something like chenopodium olidum; the false has scarcely any; the taste is very striking in the false; it is intensely bitter, and lasts upon the palate a long time, whilst in the real bark, although the taste is bitter, it is not so insupportable, nor does it last. The epidermis of the true is generally covered with an immense number of lichens, which have been described by Feé in "Essai sur les Cryptogames des Ecorces Officinales;" whilst the opegrapha pelletiere and pyrenula nitida are the only ones that have been observed on the false; besides these it is found that the real Angustura, when put into water, soon becomes dry, and imbibes it readily; it is easily broken when dry; it is very light, and of a spongy texture; it breaks with a resinous and brilliant fracture; it is internally of a yellowish brown colour, and capable of being detached in laminæ, and it is easily cut by sharp-edged tools. Now, in contradistinction to these characteristics, the false bark, put into water, still remains hard; it is difficult to break; it is very heavy and compact in its texture; when broken, it is unpolished and black; internally it is nearly black, smooth, and not to be detached in laminæ, and it is cut with difficulty. Chemistry, likewise, comes in to our aid. She detects oxide of iron in the false and not in the true, for the ferrocyanate of potash causes, in a muriatic infusion of the false bark, a precipitate, which is first green, and then becomes blue; and the same reagent converts into blue the reddish powder which lines the bark; no such effect is visible where the true Angustura bark is thus acted upon.

It appears that the active matter of the bark is taken up by cold and hot water in infusion, and that it is not injured by boiling; for the infusion we are directed to take of cusparia, bruised, five drachms, of boiling distilled water, a pint, macerate for two hours in a vessel lightly covered, and strain; of this the dose usually prescribed is from one to two ounces. Dr. Anthony Todd Thomson found that the watery infusion precipitated the infusion of galls and of yellow cinchona, that it precipitates sulphate of iron, tartrate of antimony, sulphate of copper, acetate and superacetate of lead, muriate of mercury, and pure potash, yellow; he confirms the analysis of Vauquelin, which shows that cusparia bark contains cinchonia, resin, a peculiar variety of extractive, carbonate of ammonia, and essential oil. From personal knowledge I can say nothing of the Angustura bark. Brande, who wrote in 1791, was very sanguine as to its very great efficacy. Alibert gave it a fair trial at the *Hospital of St. Louis*, but does not seem to have appreciated it very highly. It seems to check diarrhœa, and to be useful in some stages of dysentery, and may doubtless be employed where tonics are indicated.

CONTRAYERVA, DORSTENIA CONTRAYERVA.

The contrayerva has been, in its day, a fashionable medicine, but I believe that it is rarely ever prescribed in modern days. In the former Pharmacopœia there was a compound powder of contrayerva, made of five ounces of the powdered root, and a pound and a half of prepared shells, of which the dose was from ten to thirty grains in water, or rubbed up with mucilage and mint-water; it was stated upon good authority to be of advantage in typhoid fevers, in the malignant exanthematic, in chronic dysentery, and diarrhœa; it was a medicine of the school of Huxham and of Pringle, but Dr. Cullen seems to have driven it out of repute, and the present Pharmacopœia has erased from its formulæ the powder which it appears was often rubbed into balls, and administered under the name of lapis contrayervæ. The plant from which it is obtained is perennial; is a native of South America and some of the West India Islands. Dr. Houston asserts, that the roots of two species of this family are often mixed and exported with that which is called officinal, the dorstenia houstonia, and drakenia. It was placed by Jussieu amongst the urticeæ or nettle tribe, and is of the

Linnæan class and order, Monœcia Diandria. The roots are the parts which have been medicinally used; they have a bitterish taste, a remarkably overpowering odour, with some degree of purging; where they are indigenous they are considered to have much power as antidotes to the bites of venomous animals; water and alcohol both extract whatever power they possess; the watery decoction is highly mucilaginous, and was a favourite remedy as a gargle in malignant angina and in scarlet fever, both with Fothergill and Withering. It has been recommended too in dropsies; amongst the herbalists it is often quoted; Motherby and Hill recommended a tincture, but they observe that the old prescribers put some warm root into it, such as the galangal, and it then forms an excellent cordial and sudorifle: "good in fevers and in nervous cases, and against indigestions, colics, and weaknesses of the stomach."

STATE OF MEDICAL SCIENCE IN SPAIN.

To the Editor of THE LANCET.

SIR:—The high estimation in which the medical profession is held, both in France and England, induces me to contrast the deplorable want of respect evinced towards it in Spain.

When the British Auxiliary Legion went out to Spain in 1835, it was notorious that the medical officers (generally speaking) had the worst billets, and, with but few exceptions, received the most cavalier treatment from the Spaniards. So well aware was Lieut. Colonel Sloane, who commanded the 4th, or Queen's own Fusileers, from experience, that the Spaniards thought little or nothing of medical officers, that he ordered the surgeons and assistant-surgeons of his regiment to wear the sash, so that they should not be distinguished from other officers.

The surprise which this want of respect created in my mind, was completely eradicated on perusal of the following advertisement, which appeared in a Madrid paper: "Wanted a medical man, who will undertake to attend a parish four leagues from Madrid; the salary is 4000 reals (about 40*l.* sterling) a year, out of which he is to pay 375 reals for the maintenance of the poor. His keeping a barber's shop, however, will prove no detriment to his capacity for the office, as it is understood that he will be at liberty to make what he can by shaving! He must, however, be a chartered member of the Madrid Faculty."

It may be necessary here to observe, and I have it upon the authority of two Spanish medical practitioners of San Sabastian, that one year's attendance on the lectures at Madrid, entitles them to practice in the provinces.

For the edification of your readers, I will lay before them *two cases*, the one medical, and the other surgical, to show what knowledge these gentlemen possess.

Annetta Y——, æt. 9, daughter of a wine-shop keeper in San Sebastian, was attacked with typhus fever, and allowed to remain some days before medical aid was sent for, the mother thinking the child had nothing but a cold; on the fifth day, however, she became alarmed, in consequence of the patient's pallid countenance, emaciated form, and her constant refusal of food, and immediately sent for a Spanish physician, who ordered some chocolate to be given to the child immediately, the door of the bed-room to be closed, the head to be bound up with a handkerchief as tightly as she could bear it, and to have at night a little opening medicine.

The next day he called, the child was considerably worse, and had, during the night, pulled off the handkerchief repeatedly, and complained of intolerable pain in the head; the handkerchief, however, was directed to be tied tighter to stop the pain! Food was ordered to be administered repeatedly in small quantities; the child loathed it, but she was forced to take it according to orders!

On the evening of the seventh day from the attack, I was sent for *by mere accident*, in consequence of my dealing with a Frenchman who lived on the flat above Signora Y., and who came and requested me to see the child; I accordingly accompanied him, and, on entering the apartment, I was horror-stricken; the poor child was lying in a recess, with a candle burning at the top and another at the bottom of her bed; a crucifix in her hand, head bound up; twenty leeches on her chest; blisters on the calf of each leg, and on the soles of the feet. The mother and father were standing about the bed of their only child, together with their relatives, crying with the utmost bitterness. The room was lighted up with long wax candles, a custom practised by the Catholics abroad, when any one is lying dangerously ill. The effluvia in the room was *intolerable* from want of ventilation, and the fæces being passed in bed, the sheets of which had not been removed for three days! On seeing the dark-brown dry tongue, and the sordes about the mouth and teeth, I at once recognised my familiar acquaintance, *typhus fever*. I at once ordered the head to be unbound and shaved, the leeches and blisters to be removed, the child's linen and bed-clothes changed, and the room to be freely aired:—the relief which these simple means alone afforded the poor little creature is not to be described; the room was abundantly sprinkled with chloride of lime water.

Having exacted a promise from the parents

of the child, that the Spanish physician was not to interfere with my treatment, I ordered the following—

Nitrate of potash, 8 grains;
Tartarised antimony, ⅛th grain;

to be given every four hours; the head to be constantly kept cold with a napkin well moistened in water, lemonade to be given as drink, and no food whatever to be administered, *not even chocolate!* My orders were listened to very attentively, till I gave the last, when the poor mother exclaimed—"You wish to kill my child! she has eaten little or nothing for nearly a week! and without food how can she live?" I lulled her fears by informing her that, in this disease, patients had no desire for food, and that when I had the fever at Santander, I was for upwards of sixteen days without any kind of food, and had no other drink but lemonade. The next day when I called, my patient was comatose; the pulse was like a thread, and running like a race-horse. I immediately applied a blister, extending from behind the ears to between the scapulæ, exhibited small doses of camphor and musk, and ordered hot Xeres wine to be given frequently in small quantities, having had various opportunities of judging of the advantages resulting from this mode of treatment.

Previous to my departure, the mother took me aside, and said that the Spanish physician had been to see the child, and expressed his regret at the unnecessary torture I was putting the patient to, as there was not the shadow of a hope of its recovery. She wished, therefore, to know from me distinctly, "whether the child would die or not?" I told her that the child was certainly as dangerously ill as she possibly could be, but that *I had seen cases* quite as severe, recover, and, although I could not conscientiously answer her question in the affirmative, still I was bound to leave no means untried.

On the second day after the application of the blister, my patient was rather better; I discontinued the wine, and commenced with the following—

Solution of acetate of ammonia, 1½ ounce;
Solution of tartarised antimony, 1½ drachm;
Peppermint water, 8 ounces. Make a draught. A wine-glass to be given every four hours.

After exhibiting this mixture for a few days, I found I could, with safety, give my patient small quantities of food, a tonic occasionally, and wine twice a day. About five weeks from my first attendance, I had the unspeakable joy of seeing my little patient quite convalescent, accompanied by her mother, enter my quarters. The mother called to know what remuneration she could make me; I declined taking any money, but accepted a gold ring which she presented me with.

When the action of the 15th of March, 1837, took place in front of the lines of San Sebastian, one of the volunteers of Guiposcoa had his left leg, below the knee, completely shattered by a 32-pound round shot; his leg was merely hanging by a shred; the poor fellow was carried, by our hospital transport men, to the place where the Spanish ambulance was stationed. Two Spanish surgeons began a consultation about the part *where* the leg was to be amputated; this lasted about *ten* minutes; the preparation for carrying it into effect lasted about quarter of an hour, for Spanish surgeons (as far as I have had opportunities of judging) invariably apply a linen bandage round the limb, *to indicate* the first incision. The two men of our hospital transport corps, who conveyed the man to the Spanish ambulance, having been in the habit of seeing things done in a different style at the Legion hospital of San Telmo, left the place in disgust, giving vent to such feelings as then possessed them. I rode off, shortly after, to see the action, and did not return *under three-quarters of an hour*, and they were *only then sawing through the bone*. It struck me, from what I observed on my return, that the delay was occasioned by an *awkward management* of the shattered part of the limb.

Spanish surgeons are, without exception, the coolest operators I ever saw in my life; for, when engaged in an operation in their own hospitals, they are generally to be seen with a paper cigar in their mouths, swearing at the cries of the patient, or at any mishap that may occur during an operation. Nor are they by any means proud of their position, for they resign the knife to any professional bystander who is willing or anxious to operate; once only did I avail myself of their courtesy, from the example of other medical officers of the Legion who had done so before me.

I have stated these two instances to show the comparative advancement of medicine and surgery in *Spain* and its neighbour *France*, and, if you deem them of sufficient interest to your readers, they are quite at your disposal. I am, Sir, your humble servant,

WILLIAM LAMBTON, M. R. C. S.
Late Surgeon unattached B. A. L.

96, Sloane-street, Jan. 29, 1838.

THYMUS GLAND.—Complete atrophy of the thymus gland does not take place in the child before the age of twelve years. A remnant of this body has been found as late in life as fifty years.

THE LANCET.

London, Saturday, February 3rd, 1838.

CIVILISED nations have universally adopted the practice of subjecting candidates for degrees and licences to practise medicine as a profitable occupation, to some preliminary examination, compelling them to furnish proof of possessing a certain degree of medical knowledge and mental capacity. Some chartered companies and trading bodies also formerly prescribed an apprenticeship, and demanded evidences of ability and acquirement from those individuals who sought to become members of their fraternity; and, as is too well known, under the pretext of protecting the public against incompetent dealers, they extorted large sums of money from young men who were settling in the world, strove to uphold monopoly in all its odious forms, and, instead of promoting, retarded the progress of their respective arts.

Whenever the medical corporations have exhibited the same corrupt tendencies, have laid schemes of personal aggrandisement, have thrown vexatious obstacles in the way of the medical student, have exacted heavy fees, or have sought to deprive the public of the services of meritorious individuals, the independent portion of the medical press has denounced their fraudulent designs, and the result of its labours has been that no narrow views of medical policy can much longer be sustained, nor can any new attempts to shut out well-qualified men from the ranks of the profession, unless they contribute to the treasuries of the corrupt governors of the colleges and companies, for a moment hope to succeed. A rational system of education recognises no such absurd illiberalities. It will allow a high standard of knowledge to be fixed, and rigid tests of practical skill and scientific attainments to be established; but under it the administrators of the law would be enabled to demand nothing more,—no proof of having studied at Oxford or St. Andrews'; in London or at Aberdeen,—no certificates in testimony of a "diligent attendance" on the prosy lectures of Professor A., or of probable somnolence at the discourses of Mr. B.,—proof neither of "circumcision," nor of "uncircumcision,"—of orthodox credence in the Thirty-nine Articles, or of ability to construe a few verses in the Greek Testament, before the Protestant and Worshipful Senate of a London University.

The right of the Legislature to prevent ignorant and incompetent persons from treating the sick, is founded on the basis that security for life is one of the great ends of civil government. Criminal law, and the vast apparatus which is requisite for its administration, have been established in order to deter assaults on life, and the destruction of property, the product of labour, as labour is the offspring of health. To show the relative injury which will arise from the indulgence of personal violence in a community, and the evils which are likely to occur from allowing ignorant persons to aggravate, or trifle with, diseases, it may be stated that the number of individuals who were committed to jail in England and Wales, on charges of murder, during the ten years which were included between the years 1824 and 1834, amounted to 672. The number committed for shooting at, and stabbing, &c., during the same period, was 973. For manslaughter, 1581. For infanticide, 232. For all these crimes a total of 3379. The total number of deaths produced by diseases, and all other causes, amounted, in the ten years named, to three millions and a quarter. For every one person committed for *murder*, 4836 died from other causes; and in Sweden, where the number of murders is accurately registered, there was (in 1805 to 1830) one murder in 5384 deaths; or, including cases of infanticide, one murder in 2868 deaths.

The three and a quarter millions of deaths in England and Wales implies at least 160 millions of cases of sickness. Let it be admitted, first, that under *efficient* medical

treatment 3 millions would have died; secondly, that, if left to *Nature*, 4 millions would have perished; thirdly, that if the whole 160 millions of unfortunate patients had been placed at the mercy of quacks and uneducated prescribing-druggists, or other pretenders, the fatality would have been increased, either by a frustration of the efforts of Nature, or the administration of unsuitable medicines, to 4½ millions. This is a moderate computation, but after every possible deduction has been made, the deaths which are likely to occur from the ill-treatment of the sick by quacks and prescribing druggists, throw into shadow all the murders and all the deaths from violence that could happen in any state of society, the murders of war being excepted.

A murder, it is true, becomes speedily notorious; the cry for vengeance upon the shedders of human blood is loudly and widely heard, while persons whose deaths are wrought by the quack and the medical pretender, fall silently into the grave, and are lost, unheeded, amid the numbers which no skill can preserve. Many fatal events, indeed, are consummated by misadventurous patients themselves, in prescribing for their own diseases, unfortunate in their own judgment, and unfortunate in their friends. The quack, or the prescribing druggist, will tell us that he visits his patients from motives of humanity, and not from mere love of lucre. But what is the result of his conduct? A drowning man vociferates for "help!" A knave intrudes in the garb of the *Humane Society*, and, seeming to "help," prevents efficient aid from being rendered by others, and the victim of his imposition perishes, plunged beneath the merciless waters.

A distinction will be drawn between an instant murder, committed by violence, and a slow, or a sudden death, under the hands of a prescribing druggist, pretender, or quack. In the latter case, the patient voluntarily places his life at the disposal of an ignorant person; no *violence* is perpetrated. But can the community sanction such an act of suicide? Should a government, professing to represent the intelligence and the humanity of the nation, suffer men to play such desperate games with their own lives? or allow reckless mercenaries to swindle the poor, the ignorant, and the superstitious, by impudent lies and cajolery, out of existence?

If any measure were brought before Parliament for the *entire suppression* of quackery, objectors would arise, and ask if the attempt were not an infringement on the liberty of the subject,—whether an impotent old man might not, if he chose, use "Balm of Gilead," or a young man "Antisyphilitic Drops," or one of the other sex consult a druggist on the mysteries of the "turn of life," and feminine disorders,—without the interference of Parliament,—or whether the people should be denied the use of "aperient pills," or prohibited from "purifying their blood," by legislative enactments? And, in the event of suppression, "what "was to become," it would be asked, "of "the thousands of prescribing druggists, "the vendors of patent medicines, the "quacks, and other such practitioners of all "descriptions?"

> Ambubaiarum collegia, pharmacopolæ
> Mendici, Mimæ, Ballatrones: hoc genus omne.

These inquiries develope two circumstances on which the existence of quackery depends,—the propensity of one part of mankind to indulge in extravagant follies, and the interest of another class in furnishing aliment to those follies. Resting on these, unhappily too secure, foundations, quackery can no more be entirely suppressed than theft; but it may be greatly diminished if the law, instead of sanctioning, *forbade* the sale of quack medicines, and punished quacks and pretenders, in whatever garb they might be found. The public would, assuredly, sanction any regulations which, while they secured an abundant supply of able well-educated practitioners, protected the ignorant and the weak-minded from the arts of death-dealing pretenders; for, although many quack nostrums are inert and innoxi-

ous, others are violent in their action, and in the highest degree dangerous.

The great mass of chemists and druggists in England receive no professional education; they are ignorant of the scientific and practical parts of chemistry, and they have no general acquaintance with pharmacy, as it is understood in Germany and France. To say that they have not the slightest knowledge of the human frame, or its diseases, is unnecessary. The only means of improving this important department of the healing art, would be to subject every chemist and druggist in the kingdom to an examination in pharmacy. This might have the effect of producing a large and respectable body of *pharmaciens*, such as exist in France and Germany, well qualified to prepare and compound medicines. Without any organisation, any assistance, any stimulus, such as prevails on the Continent, there are many trading men and dealers in this country, who are well versed in the science of chemistry and its processes, and these, with assistance from other chemists, professors of the science, both in and out of the public institutions, might afford the means of forming a department of pharmacy in a NATIONAL FACULTY OF MEDICINE.

Before advancing any specific proposals for the suppression of quackery, it would be necessary to examine carefully its causes and present state in the three countries. But this investigation we do not intend at present to make. We are not inclined, however, to let the present opportunity pass for offering a remark on the petition of the *Leeds Medical Society* on this subject, an analysis of which appeared in the last number of THE LANCET.

The subscribers to that document say that "the Apothecaries' Act is totally in-"adequate to redress their grievances;" and, in proof of this allegation, they state that in Leeds "the number of prescribing "druggists, and other illegal practitioners, "*considerably* exceeds that of the regular "members of the profession there resident;" and that the same calculation *is* applicable to many other places. We admit the soundness of the opinion and the existence of the facts, but we are surprised at the recommendation of the Leeds Committee on Quackery, that a clause embodying their wishes respecting a summary process before a magistrate, should be added to the Apothecaries' Act. Why, the Act of 1815 is one great source of quackery in England. The Fellows of the *College of Physicians* have been educated at Oxford or Cambridge; the Licentiates at Edinburgh, Glasgow, Aberdeen, or St. Andrew's; the surgeons in general practice chiefly at the London schools. This diversity of *place* constitutes the only professional difference amongst the various practitioners of medicine in Great Britain. Yet the Act of 1815 incorporates the general practitioner with drug-dealers; makes it one of his duties to dispense the medicines prescribed by his inferior in a practical knowledge of medicine, the physician; and threatens the infliction of successive penalties of five pounds, ten pounds, and loss of his "licence to practise," for refusing to prepare any articles in the Materia Medica which are directed by a legally-qualified DUB. At the same time it provides for the appointment of a court of old women, who, under the business-like title of "Examiners," are perfectly obscure as individuals, and totally unknown as contributors to medicine, but yet possess the power of rejecting from medical practice in all its branches, or stamping with the despised name and brand of "APOTHECARY," even the best educated and most talented youths of which the kingdom can boast!

This subject, of necessity, must undergo farther discussion. The present remarks have been elicited from us in consequence of the views which are expressed in the communication of a correspondent at Leeds, whose letter will be inserted in the next number of our Journal.

ONE of the most striking characteristics of mankind is the neglect which is manifested in taking advantage of important dis

coveries. It would appear that we cannot escape from the common path of custom, without making an effort which the mind is not readily capable of accomplishing. If an improvement be acknowledged, if it be obvious to the dullest perception, if it be founded on strictly scientific principles, then, generally, is the indisposition of the public to take advantage of it the most strikingly displayed. If, on the other hand, there be some mystery in the affair,—some secret which is closely concealed, like the singing process of St. John Long, then is the imposition called a "discovery," and sought after with a most energetic but morbid curiosity. Thus, quackery and fraud continue to flourish in these our enlightened modern times, while the improvements which are positively effected by scientific discoverers, are neglected, or altogether forgotten.

The unobtrusive labours of men of science carry with them but few charms for the public eye. Hence it happens that the discoveries which are of the utmost utility to society fail, in the first instance, to obtain for their authors that reputation and honourable reward which they so richly merit, and they are frequently only brought into use as the prejudices of the generation in which they were made, slowly and almost imperceptibly subside.

The refusal, during the past week, of a hospital surgeon to perform the operation of dividing the tendo Achillis, for the cure of club-foot, accompanied by a declaration that he was "no admirer of the modern improvements in surgery," furnishes a lamentable instance of the pertinacity with which even men of science will sometimes adhere to the forms and usages of long-established custom, and with what extreme reluctance it is that the thoughts are directed into any newly-discovered channels.

Now, what is the history of the operation for the cure of club-foot, which, although one of the most simple and useful amongst recent improvements in the art of surgery, has not been practised in proportion to the advantages which it is calculated to confer? The operation is one which every surgeon is competent to perform, and removes, in a quick and efficacious manner, a very disagreeable and distressing deformity to which the name of a late noble poet has given a great degree of notoriety.

The deformity is, generally speaking, congenital, but may be acquired. It may exist in one or both feet. It sometimes consists in a simple retraction of certain muscles of the lower extremity; in other cases it is complicated with a deviation of the foot to the inner or the outer side. The first form is denominated "pes equinus;" the second, "varus internus vel externus." It is unnecessary to explain at length the pathological anatomy of this affection. It will suffice to say, that an analysis of the causes by which the foot is retained in the different abnormal positions described, proves that the principal indication of cure in cases of club-foot is to elongate the muscles by which the foot is forcibly retained in its distorted position. For this purpose various machines have been invented. The practice of the late Mr. Sheldrake consisted in the employment of frictions, which were regularly continued over the entire course of the gastrocnemii muscles, and, by bending the foot with the hand, endeavouring, from day to day, to extend the heel to its natural position. This treatment was adopted in a great variety of instances with the most signal and complete success; but the tediousness of the process has prevented its general adoption. Several cases were related by Mr. Sheldrake, in his lectures, which were published in The Lancet for 1828-9, vol. i., and one of the cases therein recorded, that of John Robinson, at page 278, vol. i., fell under our own observation. The effect of the friction and extension used in *that* instance, was extraordinary, and the removal of the deformity peculiarly rapid.

The treatment by irons or machines, however, is tedious, often perplexing, gives rise to severe pain, and, in a certain number of cases, fails altogether to produce relief

From these causes a different mode of cure was deemed desirable, and was at length found in the division of the tendo Achillis. The first case in which this operation was tried for the removal of club-foot, was that of a young girl, 17 years of age, who was said to have suffered under the deformity from the time of birth. The operation was executed in the year 1784, by a surgeon, named LORENZ, at Frankfort, under the direction of a physician, to whom the idea of dividing the tendo Achillis first occurred. A cure was effected in about six weeks. This example, though worthy of being followed, did not find a single imitator, and the proceeding fell into disuse, until it was revived in the year 1811, by a physician at Marbourg, who, however, committed the error of dividing only a portion of the tendo Achillis. In the year 1812, SARTORIOUS operated on a boy, 13 years of age, and, by the aid of proper machinery, obtained a radical cure in a very short space of time.

In all the operations to which we have now alluded, the skin was divided, as well as the tendo Achillis. Hence, when the heel was brought down, the edges of the wound were forcibly separated, and troublesome accidents supervened. To remedy these inconveniences, M. DELPECH proposed a new method, which he executed on the 9th of May, 1816, and which consisted in dividing the tendo Achillis, from before backwards, with a convex bistoury, which he passed in front of the tendon, from one side to the other. As he cut from before backwards, the whole of the tendon was divided, without any injury being done to the integuments which cover it on the back of the leg. Instead of bringing down the heel immediately, and applying machinery, DELPECH permitted the ends of the divided tendon to unite, and then endeavoured, by gradual extension, to elongate the intermediate uniting substance. This was a tedious and painful process. Extensive inflammation, followed by suppuration, set in, and the wounds were not healed after a lapse of three months. The deformity, however, was very considerably relieved, and the patient now walks with perfect ease, and is capable of undergoing considerable fatigue, while the shortening of the foot does not exceed a quarter of an inch.

The imperfect degree of success which attended the only operation for club-foot that DELPECH ever performed, discouraged even that distinguished surgeon, and division of the tendo Achillis was again forgotten until the year 1833, when M. STROMEYER, of Hanover, revived it, with some useful modifications. Instead of making two perpendicular incisions, on either side of the tendo Achillis, after the manner of DELPECH, Dr. STROMEYER passed a small, narrow-bladed bistoury, from one side of the heel to the other, between the tendon and the bones, and then, having directed the edge of the knife backwards, divided the tendon without injuring the integuments, excepting at the place where the instrument entered.

Another point in which STROMEYER deviated from the method of DELPECH, was, that he commenced the application of machinery three, four, or five days after the division of the tendon, whereas DELPECH allowed not less than twenty days to intervene between the section of the tendon and after-treatment. Dr. LITTLE, who has paid much attention to the subject of the "club-foot," and the means of remedying that deformity, and whose first publication on the distortion appeared in THE LANCET, vol. ii, 1836-7, considers the Stromeyerian method to be the best; but M. BOUVIER, a French surgeon, well known for the improvements which he has introduced with respect to the treatment of deformities, commences extension of the foot immediately after division of the tendon, a practice which, in his hands, seems to have been attended with prompt and complete relief. The machinery generally employed for the after-treatment of club-foot, is that originally recommended by SCARPA, which may be obtained at the house of any surgical instrument maker; but in the country, where it is sometimes difficult to procure the precise article which is wanted, the ingenuity

of the surgeon will readily suggest the simple apparatus that is required.

The short historical sketch which we have drawn of the operation for the radical cure of club-foot, demonstrates the truth of the principle with which we set out, viz., that in surgery, as well as in other sciences and arts, the most simple and useful improvement has frequently to struggle for years against prejudice, on the one hand, or apathy and neglect on the other, before it becomes extensively adopted; and, strange to say, the last places in which they find favour are the wards of our metropolitan hospitals; but we trust that the few observations which we have now made will aid in preventing the Stromeyerian operation from again falling into oblivion.

On Warming and Ventilating; with Directions for Making and Using the Thermometer Stove, or Self-regulating Fire, and other New Apparatus. By NEIL ARNOTT, M.D., Phys. Extra. to the Queen, Author of the "Elements of Physics," &c. London: Longman, 1838. Post 8vo.

IN this small, unpretending volume, which extends to only 138 pages of type, there is a fund of really useful knowledge which is not to be discovered in most publications of nearly twenty times the size. The subjects which are discussed in this work, relate to the ventilation of private dwellings, and of every description of public institution. Also to the best modes of warming rooms, halls, churches, and all other places in which human beings congregate either for the purpose of engaging in manual occupation, or for mental recreation and improvement.

The two subjects of ventilation and warming are treated in a masterly manner,—in precisely such a mode as we should expect to witness when the task was to be executed by the mind and pen of Dr. Arnott. The work, we repeat, is a really useful publication. It is a great benefaction bestowed upon the public. It is a contribution to health and economy, which must be highly appreciated by all the intelligent portion of the community.

Without a reference to the diagrams the text might be misunderstood; we therefore abstain from making any quotations. With regard to the Thermometer, or Self-regulating Stove, we may observe, that it has been seen by us in operation in different places, and we can confirm all that Dr. Arnott has advanced of its two great advantages,—First, that of supplying a perfectly uniform temperature throughout the whole of an apartment; and, secondly, the production of that great benefit at a cost of a most trifling amount. We know that an expenditure of a penny a day in fuel would be sufficient to maintain a temperature of 60 degrees in a room measuring 18 feet by 15. Hence it becomes a matter of certainty, in all situations where the cost of fuel is a point for consideration, that the Self-regulating Stove will be brought into operation. Divers sufferings from smoky chimneys, ill-working stoves, and defectively ventilated apartments, induce us to hope that architects will not be the last persons into whose hands this publication will fall.

As a production relating particularly to the science of health, it will, we are sure, be regarded as a valuable production by the members of the medical profession.

THE POCKET TEST-CASE FOR POISONS, AND COMPANION.

(Arranged by Bailey, Potter, and Co., Chemists, London.)

WE have much pleasure in recommending to the notice of our readers the Pocket Test-Case for Poisons, of Messrs. Bailey and Co. It will be found a highly useful and convenient appendage to the armamenta of medical practitioners, and will, we have no doubt, receive the patronage which it deserves; for, as a knowledge of medical jurisprudence becomes more diffused, professional men will naturally be more anxious to possess themselves of every means by which a discovery of the true cause of death, in doubtful cases, may be obtained.

The Pocket Test-Case does not exceed, in size, a moderately-sized dressing-case, and contains nearly all the tests which are ordinarily required for the detection of the metallic, acid, and vegetable poisons. Thus, in the list of contents, we find an apparatus for sulphuretted hydrogen, or tube retort,

zinc precipitating wire, blow-pipe, charcoal, platina foil, Berzelius' reduction tube, test-tube, green glass tube, glass rod, watch-glass, bent tube, litmus and turmeric paper copper wire. The *tests* are contained in nte stoppered bottles, holding chloride of tin; nitr. ox. of silver; nitr. ox. of barium; carbonate of potassa; hydrosulphate of ammonia; chloride of calcium; pure potassa; persulphate of iron; iodide potassium; ferrocyanide of potassium. Three small pill-boxes contain sulphate of copper, sulphuret of iron, and chromate of potassa. The strong mineral acids are omitted from the case, to prevent the effect of accidental breakage.

We have said enough to show that every medical man who may be called upon to attend a coroner's inquest, may usefully possess himself of so convenient an apparatus.

ROYAL INSTITUTION.

Friday, Jan. 19, 1838.

PROFESSOR FARADAY ON ELECTRIC INDUCTION.

This was the first evening meeting of the members for the season, and exhibited a "grand re-union" of scientific men. Dr. Faraday having made a series of experiments to ascertain the true laws of *induction* before the *Royal Society*, the results of which are now in course of publication by that learned body, repeated this evening several of the series, and presented them as a *bonne bouche* to the scientific gourmands who patronise Faraday's exquisite *cuisine*.

No science, according to the lecturer, has ever made so rapid a progress as electricity, and none has been so much retarded by the assumption of hypotheses, which the world at large have received with the most complacent gullibility. It was stated, as an indisputable principle, by Œpimus, and admitted by Cavendish and Pauillet, "that the action of electricity is in right lines, and with a force inversely as the square of the distance," like common forces: and that air, glass, or other interposing substances, act as impediments to its transmission. Faraday controverts this theory altogether, and labours by his experiments to prove, that electric induction is not produced by a force acting in right lines, but in curved or angular lines as well; and that it is not transmitted through empty space, but can alone be communicated from one body to another by the successive polarisation of contiguous molecules.

Dr. Faraday uses the term *induction* to express universally the fact, that the condition of one body is electrically changed by the approximation of another excited body. Thus, a glass tube being rubbed by a waxed cloth, and in this manner excited, when placed in the neighbourhood of a cylinder of brass or other metal, puts the cylinder into a state in which it exhibits electrical phenomena, such as attraction and repulsion of light bodies. This is an example of induction, and Dr. Faraday refers all the phenomena which have hitherto been expressed by the words *conduction*, *insulation*, &c., to this single principle.

The lecturer exhibited, with his wonted elegance and precision, a number of experiments, in illustration of the nature of the inductive process. Thus, a cylinder of glass excited in the ordinary manner, being approximated to two insulated metallic cylinders, formed like common conductors, an electric condition was *induced* in the metallic cylinders, and each of them exhibited the same electric phenomena after the glass cylinder was removed. But if the most distant of the metallic cylinders were removed before the glass rod, then the two were found to be in different states of electricity, the one being positive, and the other negative. Having also *induced* a negative state of electricity in surrounding matter, by charging a large electric battery through the medium of a powerful machine, he exhibited the force and activity with which the electric fluid rushes to an equilibrium through the medium of a small iron chain suspended in festoons. In this experiment two kinds of light were made evident; a blue light, produced by the transit of the electric fluid from link to link; and a white light, the effect of a combustion of the iron; the blue being the index of a series of inductions.

The lecturer did not exhibit any direct experiment to prove the successive polarisation of the atoms intervening between the negative and positive bodies, alleging that the experiments corroborative of his principles in this respect, and which are detailed in the forthcoming volume of the "Philosophical Transactions," were too delicate to be satisfactorily demonstrated in the presence of a large audience; but he made an illustrative experiment in this ingenious way: Having placed three small bars of soft iron upon pivots in a line, they were seen to revolve horizontally *ad libitum*, but when a magnet was placed at one end of the series, the three bars became successively polarised, and became so arranged that the positive pole of one was in contact with the negative pole of the other. When the middle bar is removed, however, the first bar alone is affected by the magnet, the third remaining perfectly indifferent. This was a kind of metaphorical experiment, to show the mode in which, according to Fara-

day, the electric fluid travelled from particle to particle of matter.

The final experiment of the lecturer, destined to prove that the electric matter did not act in right lines only, was the following:—Having excited a cylinder of shell-lac, he placed it on an insulated pedestal, and by means of an insulated brass ball, exhibited its character on a paper ball suspended by a glass thread. He then placed a disk of brass upon the top of the shell-lac column, and having momentarily touched the disk, to render it indifferent, he placed the insulated brass ball upon the centre of the disk, and having thus obtained a portion of excitement, showed by approximating it to the paper ball, that it was charged by the same kind of electricity as when it derived its excitement directly from the shell-lac; thus demonstrating that the electricity had passed from the shell-lac cylinder over the edges of the brass disk, and affording an example of what the lecturer termed "*induction round a corner.*"

At the conclusion Mr. Faraday congratulated the members upon the improved appearance of the mansion, a handsome Corinthian colonnade having been erected on its front. He also alluded to a vulgar error, which was going the round of the newspapers, "that salt was a desirable means of removing ice from the pavement." This he thought a most dangerous plan, by which dry ice of the temperature of 32 degrees, was converted into water at zero, a footing not at all agreeable in his opinion.

In the library we observed a print of the celebrated German physiologist, Müller, published by A. Schloss. He appears to be a young man, having "a fair large front and eye sublime," with features denoting strength of will.

January, 15, 1838.

MR. ADDAMS ON ACOUSTICS.

The season has commenced here very actively, three concurrent courses of lectures on different branches of physics having been established. Mr. Addams having been appointed to deliver a series of prelections on acoustics, commenced operations this day. He defined sound to be "a sensation excited in the mind by the vibrations of certain bodies, conveyed to the sensorium through the medium of the auditory apparatus." He noticed the only apparent exception to the rule that "sound is the result of vibrations," viz., the production of sound by the introduction of a flame of hydrogen gas into a glass cylinder. This phenomenon has been proved by Professor Wheatstone to be the result of vibrations excited in the contained air, in the following experiment. The flame of an ordinary taper being placed before a mirror fixed on an horizontal axis, but with its surface inclined a few degrees from the perpendicular, when the mirror is rapidly revolved, produces, by reflection on the face of the glass, an illuminated circular line of *equal* breadth throughout. But, if the flame of hydrogen be substituted for that of the taper, the illuminated circle will be of *unequal* breadth, alternately widening and narrowing at regular intervals. This appearance according to Professor Wheatstone, is owing to the alternate enlargement and diminution of the flame, by which means a vacillating light, and varying degrees of temperature are produced, the former accounting for the varying dimensions of the luminous circle; and the latter, through the alternating contraction and expansion of the air exciting the vibrations which engender the sound.

Another unique fact relating to the origin of sound—the production of vibration by the contact of a hot with a cold metal, which has puzzled the natural philosophers for some years, Professor Forbes has attempted to explain in the following manner:—A piece of copper or brass, formed in the shape of a plaster spatula, having one surface like a double inclined plane, when heated and placed with the convex side downwards, resting on a flat ingot of lead, will assume a rapid and rocking motion, attended with a humming sound. The spatula, resting on its crest, rapidly *see-saws* from side to side, the hot surface of the copper being apparently repelled as soon as it has touched the lead on either side. Hitherto this phenomenon has been explained by the supposition that the lead, being expanded by the approach of the hot copper, was thereby elevated, and repelled the copper, which, meeting with the same opposition on its other phase, was thus alternately repelled from side to side. Professor Forbes, not thinking this rationale satisfactory, has instituted several experiments, in order, if possible, to elicit the truth. There is no obvious expansion on the surface of the lead, but it must be considerable to throw back the copper to the opposite side,—and he has, after some deliberation, come to the conclusion that the phenomenon in question is due to the repellent power of caloric.

Having shown by experiment that musical sounds produce vibrations in membranes, and adduced this fact as a proof of the *modus operandi* in which the membrana tympani is affected by sound, Mr. Addams gave a distinct but general description of the organisation of the ear. He then illustrated, in a pleasing manner, the mode in which sound is conducted through various bodies, and produced the following table of the velocity with which sound travels through certain media.

	No. of Feet per Second.
Air	1130
Water	4900
Cast Iron	11090
Steel	17000
Glass	18000
Wood	4636 to 17000.

Of the woods, deal or cedar is the best conductor, and consequently most fitted for musical instruments or stethoscopes.

The influence of temperature in accelerating the transmission of sound, was exemplified by two organ-pipes of the same pitch, the mere application of the hand to one raising its pitch by a couple of notes.

Mr. Addams appears satisfied with the calculations of Laplace in relation to the discrepancy between the theory of the velocity of sound through the atmosphere, adopted by Sir Isaac Newton, and the results of experiment, and appears to attach no weight to the objections of Dr. Ritchie.

The ordinary vehicle of sound is the atmosphere. Aristotle, deriving his information from the Pythagorean school, acquired tolerably just notions of the nature of sound. Galileo also was well acquainted with harmonics, theoretically and practically; but it was reserved for Newton to sketch out the true theory of sound. In his "Principia" he explained the origin of aerial pulses, and by a fine application of dynamics he succeeded in calculating the celerity of its transmission. He determined from theory that the velocity of the undulations of an elastic medium generally is equal to that which a heavy body acquires in falling by the action of gravity, through half the height of a homogeneous atmosphere of that medium; but the actual velocity of sound in atmospheric air is found to be one eighth greater than what is assigned by that formula. This difference was attempted to be accounted for by Newton, on the supposition that the molecules of air are solid spheres, and that sound is transmitted through them *instanter*. Laplace endeavoured to reconcile the difference between theory and observation, by the hypothesis that heat is disengaged from each successive portion of air during the progress of the condensed wave. The late estimable Dr. Ritchie regarded the hypothesis of Laplace as a gratuitous assumption, the falsehood of which is apparent from the fact that a rarefied wave advances through air with the same velocity as a condensed wave, which would not be the case if in either instance their progress were influenced by the heat evolved. Dr. Ritchie has entered into calculations, to show, that if the molecules of water be assumed as incompressible, and when at the temperature of maximum density, very nearly in absolute contact, we ought, in estimating the velocity of sound in steam, to add to the velocity given by the formula of Newton, the rectilinear space occupied by the molecules which, if a cubic inch of water be converted into a cubic inch of steam, will be one-twelfth of the distance. By comparative experiments with a tuning fork, held over a tube closed at one end, and containing at one time air and at another time steam, and also by similar trials with organ-pipes of various lengths, Dr. Ritchie found a close agreement between his theory and observation. He also proved that this theory furnishes the means of determining *a priori* the density of a liquid, if the velocity of sound in the vapour of that liquid be given.

January 26, 1838.

POISONOUS CANDLES.—GOOD EFFECTS OF PUBLISHING ABUSES.

Mr. BRANDE delivered a lecture this evening on fatty matter and the manufacture of candles. A few weeks since we gave a full report of a lecture on fatty matter, delivered before the *Westminster Medical Society* by Mr. Everitt. That lecture embraced the chief topics which were dwelt upon by Mr. Brande. In mentioning the subject of arsenical candles, Mr. Brande spoke of the service rendered to the public by the *Westminster Medical Society*, from their investigation of the matter. In the commencement of the use of arsenic in stearine candles, the quantity used was exceedingly small, not more than a tenth of a grain in a single candle. When he remonstrated at the time with a manufacturer on the danger of using so poisonous a material, he was told, the doctors used small quantities of arsenic as a tonic, and the candle-makers had only adopted a new method of strengthening the stomach by the vapour of that mineral. The candle-manufacturers, however, soon increased the quantity of the arsenic, until it reached three, and even four, grains in a single candle. Regarding the statement which had been made, that some manufacturers used one pound of arsenic to twenty-eight pounds of stearine, he thought this must be erroneous, for he he had found that a candle manufactured of such proportions would not burn. From the exertions of the Society above alluded to, and the publicity given to their proceedings by the press, he was happy to say that he believed that at present there was not a single candle-manufacturer who used arsenic; it had been ascertained that a small quantity of magnesia, or fresh chalk acted similarly on the stearine, and to these less injurious substances the tallow-chandlers now resorted.

WESTMINSTER MEDICAL SOCIETY.*

Saturday, Jan. 27, 1838.

Mr. Hale Thomson, President.

HYDROPHOBIA AFTER SEVEN YEAR'S BITE.

The President laid before the Society the larynx, trachea, and spinal chord, taken from a boy 17 years of age, who had died in prison the day previous, under all the symptoms of hydrophobia. The history of the case had that morning been detailed in a clinical lecture by Mr. White, at the *Westminster Hospital*. It was briefly this. The boy had been in prison twenty-five months, and had never been exposed, during that period, to the bite of any animal; but he stated that he had been severely bitten by a dog seven years ago, and a scar remained on the right hip from that cause. During the twenty-five months of his confinement, he had always appeared sullen, gloomy, and reserved, and was never known to look the person in the face to whom he spoke. He had not complained of illness until three days previous to his death, when he became debilitated, and was removed into the Infirmary of the prison. At first, the case appeared to be that of a common cold, but symptoms were rapidly developed of the most alarming character, not unlike those of spasmodic cholera. The right hip, on which the bite had been inflicted, became excessively painful, and shooting, spasmodic twitches pervaded the whole leg, the boy constantly imploring that the most violent frictions should be applied. Sixteen hours previous to dissolution, the most decided symptoms of hydrophobia were manifested, and continued, with short intermission, until convulsive delirium closed his life. The autopsy was as follows:—The larynx and trachea, throughout its whole length, presented the appearance of acute inflammatory action; the whole of the surface of the spinal chord appeared highly injected, and of a vivid scarlet colour. In the head there was no particular appearance, with the exception of a few red spots on the surface of the brain. The cerebellum seemed full sized, and the middle lobes were large. The scalp was of extraordinary density, and the cranium somewhat thicker than usual. The stomach and intestines were perfectly healthy. The treatment adopted at the *Penitentiary* consisted in the application of blisters to the whole course of the spine, and the endermic use of morphia. Four minutes after the morphia had been used, the patient became completely calm, and remained so for a short period, when the symptoms again recurred with all their former violence.

Mr. Pettigrew referred to some particulars of cases which he had brought before the Society during a previous session. In these instances the vascularity of the trachea ceased abruptly at its point of division, into the bronchia. A similar termination of the diseased action was also observable in the œsophagus, at a point corresponding with that in the trachea. The bladder was empty, and its mucous surface inflamed. Regarding the extent of time which had elapsed from the receipt of the injury to the development of the symptoms in Mr. Thomson's case, he (Mr. P.) believed that a case was on record in which the virus lay dormant for nineteen years, and, at the end of that timer the disease proved fatal. In the cases which he (Mr. P.) had brought before the Society, the disease manifested itself in periods varying from six to eight weeks after the receipt of the injury.

Dr. Sigmond was of opinion that the case related by the President was an instance of hydrophobia occurring spontaneously, rather than from contagion. It was allied to those cases recorded by Dr. Parry, of Bath, in which every symptom of hydrophobia was present, but which gave way under the use of the lancet. This state, occurring in females, would be called hysteria, in severe forms of which affection the train of symptoms resembled hydrophobia.

Mr. Chance inquired whether any member had seen a fatal case of hydrophobia which could not be traced to a previous inoculation by a bite. Tracheotomy had been recommended in this affection, as it would afford time, at least, for the employment of other remedies.

The discussion on this case ending here, Dr. Williams related the following case:—

* The Committee of Management of this Society, and some of the members, are at issue respecting the costs of printing the late Report on Arsenical Candles. The former deny that the members who conducted or aided in the inquiry, had any warrant, under the laws of the Society, for engaging in that expense. The parties who have paid the money demand reimbursement. To night (Feb. 3) a special meeting of the whole Society will be held, and the matter fully discussed. A large attendance is expected. The cost of printing is about 25*l.*; the cost of the experiment was also 25*l.*, neither of which has yet been paid by the Society.

Perforation of the intestines coming on during the occurrence of hæmorrhage, without the acute pain usually observed in such cases, and ulceration going on to a certain extent without the presence of any remarkable symptoms.

The patient was a robust man, of temperate habits, aged 30. His health had been generally good. Dr. Williams had first seen him on Monday last; he had been suffering for eight days with symptoms which were denominated "influenza," and consisted of a severe cough, with flying pains about the bowels. The cough had been much mitigated, but the pain in the bowels still continued, with diarrhœa, which had been going on about four days. The evening before, Dr. Williams saw him as he was sitting up; hæmorrhage from the bowels came on, to the extent, as he (Dr. W.) was informed, of two or three quarts, attended with vomiting and pain in the abdomen. The next day the pulse was quicker, the skin hot, tongue quite brown and furred; the cough still continued, the tenderness in the abdomen was greater, and just before he (Dr. W.) saw him, he had lost a quart of blood; there was great exhaustion, and the bowels were tympanitic. On the Tuesday the patient seemed to rally considerably, the pain in a great measure left him; he sunk, however, in the course of a few hours. On examination, the abdomen was found to contain a large quantity of serum, and several folds of intestines were agglutinated by recently effused lymph. There were signs of much inflammation in the ileum, on taking out a portion of which numerous ulcers of various sizes were observed, all covered with a grayish kind of slough; the ulceration had extended through the mucous and muscular coats, and in one large ragged ulcer the serous coat was so far involved as to give way before a current of air passed through the blow-pipe. Several ulcers were found in the colon, and one large one near the cæcal valve; there was no other disease worth mentioning; the lungs were healthy; the rectum was not examined. The strongest purgative which had been used in the illness preceding the hæmorrhage, was hydrarg. c creta. One or two doses of calomel and James's powder had been administered for the removal of costiveness, which had preceded the "influenza."

Mr. Hardwick had seen cases of a similar nature, connected with tubercular diseases of the lungs, or the mesentery.

MEDICAL SOCIETY OF LONDON.

Monday, January 29, 1838.

Mr. Bryant, President.

ENCYSTED TUMOUR IN THE ABDOMEN.—DEATH OF MR. WHITMORE.

Mr. Robarts had seen a case last summer, which at the time interested him much. It was that of a young woman, 19 or 20 years of age, who had always enjoyed good health, with the exception of a large tumour, which was situated at the right side of the abdomen, and which she had noticed to be present from her childhood. Last summer she suffered from all the usual symptoms of typhus fever. At the end of three weeks from the commencement of her illness the tumour began to be painful, and increase in size; leeches and subsequently blisters were applied to it, but it kept enlarging, and getting more painful, and the patient, who was obliged to constantly lie on her back, sunk from hectic and sloughing of the nates. A few days before she died the tumour, which at this time occupied nearly the entire abdomen, began to point at the navel, and discharged a large quantity of very fœtid pus. On examination after death, the tumour was found to have an attachment of a hand's breadth in extent to the under surface of the liver, the peritoneal coat of which was reflected over the cyst. It had another attachment to the anterior parietes of the abdomen, which were partially ulcerated away. The cyst was capable of containing two gallons of fluid, and there were nearly three-fourths of that quantity of very fœtid pus in it. Its internal surface had been nearly destroyed by extreme inflammation, having become quite flocculent in appearance. The coats of the cyst were as thick as a penny-piece. Its connection with the liver was not dissected away, but that organ, when cut across in various parts, seemed quite healthy. The other abdominal viscera did not show any signs of disease, and the tumour had no connection with any parts but those mentioned. He (Mr. Robarts) had thought at one time of puncturing the tumour, which was always distinctly fluctuating. Was it an ordinary circumstance for cysts of this description to inflame during the presence of fever? A long and desultory conversation followed respecting the origin and nature of the tumour. Mr. Kingdon seemed to think it was originally an hydatid cyst under the peritoneal covering of the liver, and that if its contents had been accurately examined they would have been found like those found in hydatidiginous growths. Mr. Crisp inquired whether the peritoneum might not have been posterior to the tumour, which he thought might have been a collection of matter between the peritoneum and parietes of the abdo-

men. Mr. ROBARTS considered that had the tumour consisted of a collection of matter between the parietes of the abdomen and the peritoneum, the matter would have burrowed in various directions, and not have been so completely circumscribed as it was; besides, this view of the case would not account for the tumour in the early period of life.

DEATH OF MR. WHITMORE.

This gentleman, who was a respected Fellow of the Society, and one of the Council of the *British Medical Association*, fell a victim to typhus fever a week or two back. It appeared from a conversation which took place respecting his complaint, that he had contracted it at a poor-house, where he had been attending a number of persons with the disease. He had humanely assisted in lifting many of the patients up in their beds, and performed many other offices which usually belong to nurses. The fever was complicated with chest affection, for which he was bled and relieved; but the typhus became gradually more developed, and he eventually sunk from apoplexy.

MEDICO-BOTANICAL SOCIETY.

Wednesday, January 24, 1838.

Dr. SIGMOND, President.

MR. EVERITT ON CERTAIN CHEMICAL CHANGES IN COMPOUND BODIES.

MR. EVERITT delivered a lecture on a peculiar class of chemical changes brought about in compound bodies, more particularly in organic compounds, merely by the presence of certain agents, where these agents do not enter into combination with the substance acted on, either as a whole, or yield to them any part of their constituents; they appear to us, as far as our observations and their changes go, to act only by their presence. The instances which Mr. Everitt cited in illustration of this class of phenomena, were the conversion of starch into sugar, merely by its being boiled for a few hours with water slightly acidulated with sulphuric acid, which acid, after it has effected the modification, is found in a perfectly uncombined and unaltered state, and can be removed by chalk, the sugar being then left alone in solution. A similar action is effected by the same agent on the fibres of wood or lignin; thus, if linen be cut into very small pieces, and oil of vitriol dropped on little by little, carefully stirring the whole to prevent the temperature rising, afterward rubbing the mass in a mortar, it soon loses its fibrous character and becomes homogeneous, and quite soluble in water, from which, if we remove the acid by chalk and evaporate to dryness, we obtain a substance exactly similar to gum arabic. If, previously to removing the acid, the solution be kept at 212° for 30 or 40 hours, and then the acid be removed as before, in lieu of gum we have sugar, like that obtained from the starch. Here, as in the other case, no part of the acid has disappeared or been destroyed. Another instance is the conversion of sugar, when dissolved in water, and the solution kept at a certain temperature, into alcohol and carbonic acid, by the presence of a little ferment or some few other matters. The conversion of alcohol into ether and water has been proved to be a similar case, it having been shown that a definite quantity of sulphuric acid, properly diluted with water, and the process managed in a peculiar way, was capable of etherifying any indefinite quantity of alcohol; and other cases were briefly noticed by the lecturer. In inorganic chemistry, a few cases are known which have some points of analogy; thus the action of platinum in a spongy form, or even in mass, if the surface be quite clean, can occasion the combination of oxygen and hydrogen, so as to produce water, the metal itself undergoing no chemical change whatever.

LONDON PHRENOLOGICAL SOCIETY.

THE CEREBELLUM.

AT the third meeting for the session, at the Society's rooms, 75, Newman-street, the President, Dr. ELLIOTSON, in the Chair, a paper, entitled, "Facts and Inferences respecting the Development and Functions of the Cerebellum," by JOHN CLENDINNING, M.D., Oxon, and Edinburgh, F.R.C.P., was communicated by the Secretary, Dr. C. not being a member of the Society.

The author commenced his paper by remarking, "that the acceptableness to mankind of new facts and truths, often, perhaps usually, bears no true proportion to their soundness, and that the history of Gall fully bears out this observation; that metaphysicians, moralists, theologians, anatomists, and physiologists, unanimously agreed that his labours were worthless; yet now that all candid and learned physiologists admit that Gall did much for anatomy and physiology,—that he developed new views and facts of the highest interest and importance." The author expressed his conviction, that "the time is not far off when it will be allowed that Gall has contributed materially, enormously, to this reform, so much needed, and now in progress, in the method of investigating the phenomena of the mind."

The object of the author was to show that, on weighing the whole encephalon and the cerebellum alone, of 163 subjects, to ascertain the relative weight of the encephalon

and cerebellum, and the positive weight of the cerebellum at different ages, "he did not find the results confirm the doctrine of Gall, that the cerebellum is the seat of sexual desire, and parental love only." Not only in this did the author show his very imperfect acquaintance with Gall's writings, since Gall does not place parental love in the cerebellum, but in the posterior lobes of the cerebrum, and does not confine the use of the cerebellum to sexual desire; for he says, at p. 385-6, vol. iii., of his 8vo. edition, that the cerebellum may participate in the vital function of the chorda spinalis and oblongata, and yet be the seat of this peculiar animal function. But in many other particulars his misconceptions were equally remarkable. He stated that "Gall was not content with connecting and pointing out additional lights, but had his system, which he felt himself bound to establish on the ruins of former systems, and there found his difficulties to become great, if not to begin." Whereas Gall had no system to establish, but simply observed facts which formed themselves into a system.

"I defy," says Gall (p. 210, vol. iii., 8vo. edition), "those who attribute my determination of the fundamental faculties, and of the seat of their organs to caprice, or arbitrary choice, to possess a tenth part of the talent necessary for the most obscure presentiment of this beautiful arrangement; once discovered, it displays the hand of God, whom we cannot cease to adore, with wonder increasing, as his works become more disclosed to our eyes." The next important error was in supposing that phrenologists considered power to be proportionate only to size; whereas in all phrenological books power is said to be proportionate to size only, *ceteris paribus*, that is, quality being equal; and it is not from a few, but a very large amount of observations of the relation between size and power, that the seats of the different faculties have been established. He mentioned the head of Bruce as an alleged instance of the want of uniformity between talent and anterior development; but in the "Edin. Phil. Trans." its perfect conformity with phrenology was fully displayed. He mentioned, "that the face is allowed to indicate the qualities of the mind; but as these do not reside in the face, he argued that signs derived from the cranium might not show dependance of external development upon cerebral organs, not be that of cause and effect, but of coincidence only." He forgot that the feelings only, and not the individual talents, are depicted in the face; that we cannot judge whether a person is a musician, or an arithmetician, by his face, and that the development of the cranium actually depends upon the development of the different parts of the brain, which is the organ of the faculty of the mind. The author admits, however, that "of the *primâ fusie* grounds of doubt he is ill qualified to judge; that he has not given the litigated questions that attention which they would require of him, to entitle him to conclude positively concerning them." He says, "I cheerfully admit that I have seldom or ever met with a body of physiological evidence more imposing, or, I might say, satisfactory, whether we regard its multiplicity, or its variety, than that which Gall has accumulated in favour of the views entertained by him respecting the cerebellar functions."

The author produced two tables of 163 brains, the one giving the comparative weights of the whole encephalon and the cerebellum of both sexes, and different ages, with the intention of showing that Gall was wrong in his opinion, that the cerebellum was larger, relatively with the encephalon, in males than in females, and at puberty than in infancy, or old age. Gall's comparisons were between the *cerebrum* and the cerebellum; but not to dwell upon the author's admission, that his scales were bad, no conclusion can be drawn from his table, from the limited number of his observations, and especially from the fact, that not more than one or two brains were examined of some of the ages, while sixteen and eighteen were examined of other ages and sex. These remarks, also, apply to the other table, which was intended to show that the absolute weight of the cerebellum is not greater in the male than the female, nor at the virile period, than in infancy or old age; as far as it went, however, this table was in favour of Gall's views, since it showed that of all the largest brains four were males, between twenty-five and forty; one a male above sixty, and only one a female, and above sixty. Of the second size, thirteen were males, between fifteen and seventy, viz., three between fifteen and twenty; four between thirty and forty; three between forty and sixty; and three between sixty and seventy; and only four of these females, two from fifteen to twenty-five, and two from sixty to seventy. Of the third size, thirty-four were males and twelve females; the greater number of each between the ages of twenty and sixty. Of the fourth size the proportions were reversed, there being seventeen males and thirty-two females, and the youngest above ten years old. Of the fifth size there were eight males, mostly very old, or very young, and twenty-one females of all ages; and of the smallest size there was only one male, and that under ten years, and five females, all very old, or very young, with a single exception. The author did not know any of their characters. In conclusion, he stated that "he considered it very probable, if not quite certain, that the cerebellum has peculiar relations to the sexual instinct, and possibly that it has similar relations to the

parental instinct;" but he thinks it also highly probable, if not certain, that it has other functions, as yet to be ascertained, than those of sexual and parental love.

LETTER FROM THE

CLERGYMAN PRACTISING MEDICINE IN SUFFOLK.

To the Editor of THE LANCET.

SIR:—I trust that in the spirit of that impartiality which you appear to me to have always evinced, you will not permit one man to make a personal and unprovoked attack upon [illegible] without affording an opportunity [illegible] moreover, that the [illegible] provided [illegible]. The [illegible] labour, [illegible] neck [illegible] ace, [illegible] ur- [illegible] ed [illegible] dly [illegible] e- [illegible] his [illegible] per- [illegible] ari- [illegible] dical [illegible] by the [illegible] communi- [illegible] are upon a par with [illegible] medical practitioner. In [illegible] reside so many miles from my cure, Mr. Bree has committed himself in a palpable untruth; my place of residence being only five instead of seven miles from Ashfield, and I feel assured that my small congregation will acquit me of anything like neglect of my pastoral duties. I shall not now discuss the propriety of combining the medical with the clerical character. If I have chosen to depart from the general rule, I have done so under the conscientious persuasion that one profession is not incompatible with or derogatory to the other, and I have certainly the greatest of all examples before me in the practice of healing and teaching. A large proportion of my practice is gratuitous; and but for the aid rendered by me what would have become of the poor sufferers transferred to the care of Mr. Bree's assistant, Mr. De Wint, under the New Poor Law.

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Rattlesden, near Woolpit, Suffolk,
January 27th, 1838.

CASE OF THE PUERPERAL-MANIA PATIENT.

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"Mr. Richards (late M.P. for Knaresboro', the foreman of the Grand Jury, after presenting several bills to the Court), said, 'We have made an especial indorsement upon a bill, charging Isabella Hodges with the wilful murder of her infant child, as upon examination we found that she was insane.'

and cerebellum, and the positive weight of the cerebellum at different ages, "he did not find the results confirm the doctrine of Gall, that the cerebellum is the seat of sexual desire, and parental love only." Not only in this did the author show his very imperfect acquaintance with Gall's writings, since Gall does not place parental love in the cerebellum, but in the posterior lobes of the cerebrum, and does not confine the use of the cerebellum to sexual desire; for he says, at p. 385-6, vol. iii., of his 8vo. edition, that the cerebellum may participate in the vital function of the chorda spinalis and oblongata, and yet be the seat of this peculiar animal function. But in many other particulars his misconceptions were equally remarkable. He stated that "Gall was not content with connecting and pointing out additional lights, but had his system, which he felt himself bound to establish on the ruins of former systems, and there found his difficulties to become great, if not to begin." Whereas Gall had no system to establish, but simply observed facts which formed themselves into a system.

"I defy," says Gall (p. 210, vol. iii., 8vo. edition), "those who attribute my determination of the fundamental faculties, and of the seat of their organs to caprice, or arbitrary choice, to possess a tenth part of the talent necessary for the most obscure presentiment of this beautiful arrangement; once discovered, it displays the hand of God, whom we cannot cease to adore, with wonder increasing, as his works become more disclosed to our eyes." The next important error was in supposing that phrenologists considered power to be proportionate only to size; whereas in all phrenological books power is said to be proportionate to size only, *ceteris paribus*, that is, quality being equal; and it is not from a few, but a very large amount of observations of the relation between size and power, that the seats of the different faculties have been established. He mentioned the head of Bruce as an alleged instance of the want of uniformity between talent and anterior development; but in the "Edin. Phil. Trans." its perfect conformity with phrenology was fully displayed. He mentioned, "that the face is allowed to indicate the qualities of the mind; but as these do not reside in the face, he argued that signs derived from the cranium might not show dependance of external development upon cerebral organs, not be that of cause and effect, but of coincidence only." He forgot that the feelings only, and not the individual talents, are depicted in the face; that we cannot judge whether a person is a musician, or an arithmetician, by his face, and that the development of the cranium actually depends upon the development of the different parts of the brain, which is the organ of the faculty of the mind. The author admits, however, that "of the *primâ facie* grounds of doubt he is ill qualified to judge; that he has not given the litigated questions that attention which they would require of him, to entitle him to conclude positively concerning them." He says, "I cheerfully admit that I have seldom or ever met with a body of physiological evidence more imposing, or, I might say, satisfactory, whether we regard its multiplicity, or its variety, than that which Gall has accumulated in favour of the views entertained by him respecting the cerebellar functions."

The author produced two tables of 163 brains, the one giving the comparative weights of the whole encephalon and the cerebellum of both sexes, and different ages, with the intention of showing that Gall was wrong in his opinion, that the cerebellum was larger, relatively with the encephalon, in males than in females, and at puberty than in infancy, or old age. Gall's comparisons were between the *cerebrum* and the cerebellum; but not to dwell upon the author's admission, that his scales were bad, no conclusion can be drawn from his table, from the limited number of his observations, and especially from the fact, that not more than one or two brains were examined of some of the ages, while sixteen and eighteen were examined of other ages and sex. These remarks, also, apply to the other table, which was intended to show that the absolute weight of the cerebellum is not greater in the male than the female, nor at the virile period, than in infancy or old age; as far as it went, however, this table was in favour of Gall's views, since it showed that of all the largest brains four were males, between twenty-five and forty; one a male above sixty, and only one a female, and above sixty. Of the second size, thirteen were males, between fifteen and seventy, viz., three between fifteen and twenty; four between thirty and forty; three between forty and sixty; and three between sixty and seventy; and only four of these females, two from fifteen to twenty-five, and two from sixty to seventy. Of the third size, thirty-four were males and twelve females; the greater number of each between the ages of twenty and sixty. Of the fourth size the proportions were reversed, there being seventeen males and thirty-two females, and the youngest above ten years old. Of the fifth size there were eight males, mostly very old, or very young, and twenty-one females of all ages; and of the smallest size there was only one male, and that under ten years, and five females, all very old, or very young, with a single exception. The author did not know any of their characters. In conclusion, he stated that "he considered it very probable, if not quite certain, that the cerebellum has peculiar relations to the sexual instinct, and possibly that it has similar relations to the

parental instinct;" but he thinks it also highly probable, if not certain, that it has other functions, as yet to be ascertained, than those of sexual and parental love.

LETTER FROM THE

CLERGYMAN PRACTISING MEDICINE IN SUFFOLK.

To the Editor of THE LANCET.

SIR:—I trust that in the spirit of that impartiality which you appear to me to have always evinced, you will not permit one man to make a personal and unprovoked attack upon another, without affording an opportunity for a reply; and, moreover, that the accused may accuse in turn, provided there be just and sufficient ground. The man who attempts to catch his neighbour, should beware lest he run his own neck into the noose. But first, to my own defence, into which I shall at present enter no further than this: I am a legally-qualified practitioner, and was in practice not only previously to 1815, but probably years before Mr. Bree was born. Upon settling in this neighbourhood, on a small stipend, as perpetual curate of the parish of Ashfield, various reasons induced me to resume medical practice. My charges are regulated by the circumstances and situation of the community amongst which I dwell, and are upon a par with those of the nearest medical practioner. In stating that I reside so many miles from my cure, Mr. Bree has committed himself in a palpable untruth; my place of residence being only five instead of seven miles from Ashfield, and I feel assured that my small congregation will acquit me of anything like neglect of my pastoral duties. I shall not now discuss the propriety of combining the medical with the clerical character. If I have chosen to depart from the general rule, I have done so under the conscientious persuasion that one profession is not incompatible with or derogatory to the other, and I have certainly the greatest of all examples before me in the practice of healing and teaching. A large proportion of my practice is gratuitous; and but for the aid rendered by me what would have become of the poor sufferers transferred to the care of Mr. Bree's assistant, Mr. De Wint, under the New Poor Law.

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"Baron Alderson.—'Then you did very wrong, Gentlemen; you should have found the bill, and then she would have been in custody, and no further danger from her could happen. If you have evidence offered to you of a party's guilt, the question of their state of mind is not for you to determine; if you ignore bills on such grounds, you will have the community infested with all the mad people in the country.'

"The Grand Jury retired for a few minutes, and having returned a true bill, they were discharged."

This clearly shows that the Coroner was right in his direction to the Jury, the very same direction having been given by Mr. Baron Alderson, the Judge presiding in the *Central Criminal Court*, it being with the Crown (and the Crown only), as the fountain of mercy, to interfere, and order the discharge of the prisoner, should she become, at a future day, in a fit state to be set at liberty.

As a friend to the medical profession, I suggest that your correspondent, Mr. Brown, should be more discreet in his attacks, as undeserved censure redounds to praise, and his hasty observations and ignorance of the law of the land, show that he, at all events, would not be able to fill the office of Coroner, either with credit to himself, or with justice to his country. I am, Sir, your obedient servant,

A MEMBER OF THE LEGAL PROFESSION, AND A CONSTANT READER OF THE LANCET.

Jan. 24, 1838.

Our correspondent, Mr. Brown, was right in condemning the verdict of "wilful murder," in so far as it was calculated to prejudice the case of the accused female when brought for trial before another tribunal.—ED. L.

MR. ALLISON AND MR. PRITCHARD.

To the Editor of THE LANCET.

SIR:—Whenever you can allow me a little space in THE LANCET, I will say to Mr. Pritchard, of Hereford, that my intention was *not* to "nettle" him, but merely "to avoid jumping with him to conclusions." Although he still thinks that I ought to do so, I again beg leave to decline, and "to look *well* before I leap." As, however, the correspondence is before competent judges, I shall willingly *hasten* to say, that these observations will be final in the matter. I am, Sir, yours, &c.,

W. ALLISON.

Retford, Jan. 25, 1838.

UNIVERSITY OF LONDON.

"ADVANCE THREE STEPS, BACKWARDS!"—*Militia-Officer Clown.*

"THERE has been," says the "Globe" of Monday last, "an attempt, on the part of some of the members of the Senate of the *New University of London*, to introduce a sort of religious test into that institution. It has been proposed that all candidates for the degree of Bachelor of Arts shall be examined in *one of the four Gospels*, in *the original Greek*, and in *Scripture* History. The proposal was carried by a majority of one, the members being ten in favour of, and nine against, the rule. It was then suggested that, as the regulation involved at least an apparent departure from the principle upon which the University was founded, that of excluding all religious tests, the point should be referred to Government. This proposal was carried into effect, and the result has been that the Senate have been requested to reconsider their resolution. In this position, we believe, the affair rests at present."

And here it had better rest. Out of a Senate which could originate this regulation, in 1838, "what good thing can come?"

TO CORRESPONDENTS.

Anti-Assassin. Not yet. They are the mere repetitions of the stupid and malignant trash which was repeatedly printed, we understand, in that most imbecile of all periodicals, "The London Medical and Surgical Journal," which expired, from downright starvation,—if an odious thing can be said to have died that never lived,—on Saturday, December the 16th, 1837. Is "Anti-Assassin" certain? We beg of him to forward the *addition* to the "moral proof." If it be the "scoundrel" in question, he shall receive a public castigation, a public scourging, from Essex-street to St. James's-square.

Again the publication of *Mr. Edmonds'* very valuable paper on the mean duration of life among the peers of England, is unavoidably delayed. It shall certainly appear, with the tables, next week.

BOOK RECEIVED.

An Inquiry into the Nature and Treatment of Cholera; being Part I., Vol. II., of an Inquiry into the Principles and Practice of Medicine. By G. Calvert Holland, M.D., Physician to the Sheffield General Infirmary, Lecturer on Physic and Medical Jurisprudence, &c. Vol. II. London: Longman, 1838. 8vo., pp. 272.

ERRATUM.—In last week's Number, p. 629, line 30, *for* "preserved," *read* prevented.